INDUSTRIAL EXPLORERS

MAURICE HOLLAND

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INDUSTRIAL EXPLORERS

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BY

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WITH

HENRY F. PRINGLE

HARPER & BROTHERS PUBLISHERS
NEW YORK AND LONDON
1929

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DEDICATED

TO

MY MOTHER

PREFACE

THE stories here presented—of the work, personalities, and peculiarities of the nation's leaders of industrial research—were obtained, in many cases, against the opposition of the gentlemen concerned. Being scientists, few could view personal publicity except with distrust. They did not like to be "written up" nor to have their work dramatized. They did not wish to emerge from their laboratories to give interviews. That they did so at all was due to their interest in what would be called in Rotarian phraseology "the research game."

The authors managed to convince them, often after some argument, that their stories and the work they were doing appealed to many diverse groups. The college student, trembling on the brink of the Outside World and pondering a career, would be assisted by detailed, specific knowledge of the activities of the important industrial research laboratories and the men who direct them. For him, in part, the book was written. Others who would find the material of equal value, the authors suggested, were other research workers, industrial executives, students and teachers in scientific, engineering, and technical schools and,

possibly most of all, the general public fascinated by the romance behind the story of modern industrial progress.

And so the stories, until now untold, were obtained. The authors, in presenting *Industrial Explorers*, acknowledge their deep gratitude to the explorers themselves. They were, in the end, patient, kindly, and coöperative. It is our hope that they will find, in these sketches, accurate presentations of their work and truthful portraits of themselves. And that they will not resent it when, in some cases, we have been amused by some of their idiosyncrasies. Finally, of course, we make no claim that the entire field of industrial research has been covered in this single volume.

M. H.
H. F. P.

New York, September 7, 1928.

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INDUSTRIAL EXPLORERS

CHAPTER ONE

NEW INDUSTRIAL FRONTIERS

INQUISITIVE souls will journey forth on missions of exploration so long as even a square mile of land or sea remains unfamiliar to man. And when the planet on which we dwell becomes, finally, commonplace as the garden of a workman's cottage the explorers will turn, no doubt, to the upper regions and will seek to land on other planets.

But those adventurers are not the only explorers of the twentieth century. No less important, no less thrilling, often no less dangerous, is the quest of men whose work is carried on behind the walls of quiet laboratories, in corners of great industrial plants, and in institutions dedicated to the pursuit of knowledge. These are the men reaching out toward new industrial frontiers. They labor in academic calm with microscopes and delicate apparatus, but their work affects profoundly the daily lives of their fellow men.

The explorer is a figure of bold romance, a fit subject for epic poems, for narrative, and for newspaper headlines. He is a combination of sailor, soldier of fortune; jack-of-all-trades, but master of many. Not

long ago, as Hakluyt recorded in such detail, he was part pirate and part patriot, partly a dreamer and partly an intensely practical person who sought gold in the far-off places of the New World. The explorer rode his hobby on ships of the sea or of the desert. Like Peary, he struggled toward the North Pole with dogsleds and made, each day, a few scant miles of progress toward his goal. Or later, like Commander Richard Byrd, he used airplanes and all the other resources of the age of miracles and returned from the top of the world to his base within a few hours after starting.

The old-time explorer symbolizes, it seems to me, the old-time inventor, the genius in a garret who plodded toward his objective poorly equipped and alone. Often he lost the trail. Often he died before the goal had been reached. The modern explorer is a symbol of scientific research, "accelerated experience," as some one has termed it. He has every device at his hand, trained assistants and an organization that is the keynote to coöperative effort. Yet for all their differences, the explorer of an older day, the garret inventor, and to-day's explorer, the modern research worker, are brothers under the skin. Each is seeking something new. Each faces, to a greater or lesser degree, the chance of utter failure. Each seeks a goal unknown to other men.

"Exploration," wrote Commander Byrd in *Skyward*, the story of his career, "has always been a

battle between man and the elements." And this is true of scientific research as well. The question most frequently asked of the laboratory investigator, as he gives years on end to some apparently futile line of inquiry, is: what is the use of it all? And the question most frequently asked of the explorer, Byrd tells us, is "What is the sense of Arctic Exploration anyway?"

"The answer to this," Byrd writes, "must come ultimately in material results, if at all. For example, as a result of centuries of apparently aimless research we now have the telephone, the telegraph, radio, airplanes, anæsthesia, antitoxin, illuminating gas, electric lights, X-rays, automobiles, and a thousand devices that make life safer and happier. Every one came suddenly and seemed to be the work of an inspired inventor. But that was not true. Each was the culmination of generations of plodding abstract inquiry into the unknown and more often than not the inquirer was jeered or feared for being a necromancer.

"Exploration is just such an inquiry after abstract knowledge."

The story of industrial exploration is as striking as the attention-compelling lithograph of the latest "feature" picture, as dramatic as a mystery play, and as colorful as a futuristic jacket design of a best seller. Industrial explorers are the research workers on the frontier of industry. Impelled by the spirit of

research to push beyond the borders of the unknown, these explorers open up new territory with the tools of science, stake out claims to their discoveries, and consolidate with practical application the new position in the advance. They are, too, trail blazers on the path of progress; and their pioneer work builds the foundation of the road which connects the outposts of industry with the main highways of commerce.

The drama of industrial exploration is written in the fabulous wealth which lies hidden all about us—just beyond our reach. In the earth beneath our feet, in the air we breath, a lump of metal, a handful of chemicals—the mother lode of pay dirt remains secure—until some explorer makes a discovery! Then another "miracle of science" is emblazoned in headlines of the press. Some patient plodder is dragged out of his laboratory into the spotlight of publicity. He is fantastically pictured as a genie of the magic lamp of Science—the modern successor to Genius in the Garret. Yankee genius no longer plays the leading rôle in the drama of modern industry. Research, clothed in the romance of science and colored by the picturesque setting of the laboratory, has become the adventurer on the frontier of industry. Our independent inventors have taken their cue from the trend of the times and have joined the ranks of the Big Parade of modern industry—as research workers.

The Big Parade of modern industry is more than a figure of speech as far as the research worker in the ranks is concerned. Like a private in the army, the research worker is a unit in the organization, his equipment is modern and technical, his training is that of a specialist. The army is mobile—ready to move forward at an instant's notice. The American army of research workers numbers about 30,000 and is kept in the field at a cost of nearly half a million dollars a day. Equipped with the facilities of more than a thousand laboratories, this army is advancing the industrial frontier by creating new industries, developing new processes, lowering production costs, and stemming the tide of foreign competition in the battle for markets in every corner of the world.

Coöperation is the rule of the day. No single inventor, independent or otherwise, could have developed transatlantic telephony, much less brought it into successful commercial operation. This was the product of organized effort, a classic example of group development and characteristic of modern research.

It is the research executive, the Master Explorer, who charts the unknown area by an analysis of the problem, who assigns specific tasks to trained workers in the various fields of science basic to the problem. Breaking down the main problem into its fundamental elements provides an opportunity for simultan-

eous attack from different angles and a method by which a combined attack can be made by scientists, engineers, and technicians each working in his special field. Each worker can do his bit in the struggle of man to control the forces of nature.

What is this thing Research?

Webster says, "Research is a systematic investigation of some phenomenon or series of phenomena by the experimental method." A less erudite, though certainly more picturesque definition, "Research is a state of mind," is attributed to no less an authoritative source than C. F. Kettering, General Director of Research for General Motors. Personally, I like to think of research as "accelerated experience." Research is the answer determined by scientific methods to the question—Why?

Research made it possible for the hand of man to control millions of volts of lightning, to create iron and steel that neither rusts nor stains, to fly at speeds that rival the hurricane. Research has touched the more common things of life as well, and because men worked in laboratories a loaf of bread is to-day more nourishing. The housewife has glass that will not break. Research and the ingenuity of man made it possible to duplicate on earth a billion candlepower light with artificial suns!

The frontiers of science are farther flung than those of industry—in at least one instance they have been extended to other planets. Dr. George E. Hale,

director of Mount Wilson Observatory, enchanted the officials of a great industrial corporation recently with a vivid description of his "laboratory on the sun." He referred to the researches in the field of pure science, which resulted in the discovery of helium in the gases around the sun. Less academic-minded workers in applied science brought the discovery down to earth in the utilitarian application of this rare gas to the building of noninflammable dirigibles which were to span continents and cross the seven seas. More recently this rare gas has been used as a cure for "bends," a temporary paralysis suffered by men working at great pressures under rivers, boring a tunnel such as that which now connects New York and New Jersey.

Turning the focus of the inquisitive eye of research from things of the highest to those of the lowest magnitude, man has seen and even heard the passing of electrons. These infinitely small units have been made to obey his command and go through their paces with the precision of West Point cadets on dress parade. With tools of science and the methods of research backed by "patient money," the cell structure of fish meat has been made as familiar as the skeleton of a skyscraper outlined against the sky and as a consequence fresh fish is as plentiful in the chain stores of Albuquerque, N. M., as it is in Fulton Market, New York, or on T wharf in Boston.

Research, now a universal tool of industry, has

made it possible for some professors working in the laboratories of the Massachusetts Institute of Technology to determine with scientific accuracy the best way to make a cup of coffee in a little more than two years.

The bakeries industry, with its origin lost in earliest recorded history of man, has turned to research as a modern method of accelerating experience and in the laboratories of the American Baking Institute chemists and dietitians write the recipes, "mechanical hands" mix and knead the dough, thermostat switches control the electric ovens which bake the bread, and mechanical engineers have devised automatic machines to wrap the modern baker's dozen. Older industries steeped in tradition and prejudice have been the last to adopt research. The development of process technology by evolution has given place to revolution by research under the economic pressure of competition.

Research has proved a formidable weapon in the new competition, the competition between industries. The war is no longer within the silk industry but between those who produce natural silk and those who make artificial silk. Two thousand trade associations are operating in the United States and they are gradually learning the value of coöperative research. A recent survey shows that ninety or more have already started, and each year there are additional ones. Less than half of the associations publish the amount

of their expenditures for research, yet those reporting present an imposing total of $25,000,000 spent annually for this division of their activities.

The importance of research as a valued tool in building the industrial structure of the United States is clearly demonstrated by recently published data estimating that approximately $200,000,000 is appropriated annually. Of that amount government research agencies are spending about one-third. The number of industrial laboratories has nearly doubled in the last seven years.

Research is the battering ram which is used to undermine the technical foundation of competitive industries. To-day's discovery in the field of scientific theory inevitably leads to application in the practical field of industry to-morrow. From the laboratories comes a discovery which may mean a new industry or losses for one and expansion for another.

We all know industry is on its way, but the important thing is to learn where it is going? Statistics, barometer charts, business cycles, bank deposits, and car loadings as indicators of the state of industry or trade are accessories after the fact. They are based on past performance. Research, on the other hand, is an industrial X-ray revealing basic causes and fundamental conditions. The work of the research laboratory to-day is the commercial product of next year.

All this has, obviously, some disadvantages. Con-

sider, if you will, the problem of the banker. How is he to tell whether the money he has invested is safe? Are industrial investments as sound as men suppose, when to-morrow almost any industry may face some new competition? The banker reassures himself by studying economic charts, business cycles, financial statements. He will even send engineers and technicians to make reports on industrial loans. But now he is learning that a new measuring rod is at hand, a survey of research methods. The day will come, and shortly, when before granting a loan, the banker will insist on asking embarrassing questions regarding the research policy of his client. For future trends in trade can be read with uncanny accuracy by those who know their laboratories.

So much for the scope and purpose of research. In the pages that follow I have attempted to tell the stories of the men who are directing the work. Throughout the land these men, all of them leaders in their fields and all holding high places in industrial research, are busy in their laboratories. The general public knows little about them. There are interesting parallels and contradictions in their work. Harden F. Taylor, for example, uses cold as a preservative while W. D. Bigelow destroys cannery bacteria with heat. Hugh K. Moore, attempting to improve a process for making paper pulp, sought to eliminate acids. But L. H. Baekeland found in acids the solution of his problem and created "something

new under the sun." Samuel Prescott, of Massachusetts Institute of Technology, called into service the scientific staff and resources of a great technical institution to explore the mysteries of the coffee bean. John A. Mathews, of the Crucible Steel Company, found that adding other metals to iron provided insurance against rust and fatigue.

What sort of men are they? It is not easy to answer. The research worker defies classification. One might say of the banker, for instance, that he is inclined to be conservative, vote the Republican ticket, be well-dressed, a little austere. The lawyer is likely to base his philosophy of life upon precedents. He looks to the past in attempting to build the future. The business man, one feels, is a man with great energy; a salesman, a force in the community. And the writer—but after all, who can generalize about the writer?

One can say, at least, that these research directors have certain traits in common, certain innate characteristics mark many of them. It is interesting to find that most of them were, during their undergraduate days, exceedingly good students. One hears Willis R. Whitney, of the General Electric, expressing impatience that so many college men find "chasing a ball" the most interesting occupation of their college days. For him, the mysteries of science were vastly more exciting. The acquisition of knowledge seems to stimulate all of them beyond anything else.

What sort of men are they? All of them are inquisitive, restless, imaginative, anxious to know why "A" is so and "B" is not. We are safe in that generalization. And they are, on the whole, tolerant, industrious men, and patient ones. They are at once teachers and students. Theirs is the duty to lead. No matter how great the scientist, he cannot succeed as a director of research unless his heart is big enough to applaud when some subordinate makes good. Even more, he must encourage his associates to do so. He must be an influence toward harmony, coöperation, teamwork. Generosity, then, is an attribute common to all of them.

What sort of men are they? Well, some are well-dressed, poised, and suave, at home in any gathering. Some are young and some are old. A few feel somewhat ill at ease whenever science is not the main topic of conversation. Some can discuss Debussy, Kant, or art, music, antiques or amateur photography, the movies, and jazz. Occasionally there is one who is an excellent salesman. A few are shy and are unable to deal with men of the business world, impractical, academic, and as absent-minded as the traditional professor. The composite picture of the research worker is yet to be drawn, unless, of course, it is to be found in the sum of the chapters that follow.

W. R. Whitney

CHAPTER TWO

A MOLDER OF GENIUS

Willis R. Whitney

Willis R. Whitney, of the General Electric laboratories at Schenectady, N. Y., is undoubtedly the best beloved of all the remarkable men who are the crowned heads in the world of research. One says this without hesitation or fear of contradiction, for it seems to be the unanimous opinion of all his fellow monarchs. Many a scientist, scheduled to make the address of the evening on some solemn occasion when a medal is being presented, has intended to hold forth on Whitney's technical achievements, but has dwelt for an hour, instead, on the man's human, lovable qualities. In another decade or so, perhaps, Whitney will close his desk and will retire to his farm on the Troy road, there to continue private explorations into the realms of truth. If he does, and if a farewell dinner is given by his associates, the orator of the evening will have the unenviable task of finding something new to say. Flowers are invariably tossed when Whitney is spoken of.

He is taller than the average, quick in movement,

with dark hair beginning to grow thin and eyes that reveal inner energy, vitality, and joy in life. He seems always to be in an enthusiastic glow about something—science, truth, education, the cosmos, turtle husbandry, some experiment of promise. So marked is the simplicity of his manner, and so hearty and informal his greeting, that it is difficult for the casual visitor to realize that this is a world-renowned scientist, the recipient of many degrees and medals, a man whose words are respected in the gatherings of grave and distinguished men. Not until he has been talking for a moment or two does it become clear that he thinks with the cold, bright logic of those who build dreams only on demonstrable facts.

Among all the traits men treasure in their fellows there is none more enduring than generosity. This Whitney has to an almost fanatical degree. He is constantly sought by men from the factory or engineering departments for his opinion, advice or other help, and he always gives freely, without showing the eagerness he feels to get into the laboratories, to observe and talk over the experiments going on there, where he feels his real work to be. There is not an experimenter on the staff who has not felt the stimulus of his interest, the encouragement of his understanding, the helpfulness of his pointed suggestion. It may be only his suggestion, born of knowledge and broad experience, that has made possible the solution of some knotty problem, but sel-

dom, indeed, is this known except to the man who has received it. But, you say, this urge to help is only part of his job. Then what about the man who writes to say that he has a good opening for a competent chemist and would appreciate Whitney's suggestions? Invariably, if the opening is an attractive one, that man is invited to come to Schenectady to become acquainted with all the men there who might fit the position. "I don't want to lose Smith, Brown, or Jones. They are doing good work here. But if you can persuade one of them to leave he will go with my blessing," is apt to be Whitney's generous response. Rarely, be it said, is a man persuaded, but the desire to help his men to greater opportunities, either within or without the organization, is never lacking. There is here a fundamental generosity of the spirit that makes for better understanding in every contact. It is this quality that prompted Whitney to say, in effect, in initiating the work of the laboratory, here is one industrial research laboratory which will welcome visiting scientists and will publish its experimental results just as rapidly as they can be published without injury to the interests of the company's stockholders.

Whitney's popularity, then, is based partly on his generosity. Obviously he is a busy man, but he always has time to see the most unimportant caller. "Come in, Rain or Shine" is the sign lettered on the door of his private office, an invitation upon which

he insisted against the wishes of his secretary and assistants. He sincerely dislikes personal publicity, but the magazine writer or reporter who says that an interview can be sold for a good sum is not likely to be disappointed.

"The chap said he could get $100 for it," remarked Whitney, a little plaintively, after one long session with a rather importunate reporter. "What else could I do?"

Whitney picks good men and loads them up with all the responsibility they will carry. But he himself is the first to enter the laboratory in the morning and the last to leave it at night. A tireless worker, a constant source of inspiration for his associates who work, not for him but—with him.

Whitney's generosity, his quick sense of the ludicrous or absurd, and a certain assumed irresponsibility combine to make him beloved. But the universal respect in which he is held by his fellow scientists in this country and abroad is based on his profound knowledge and on the manner in which he inspires his colleagues to new bursts of endeavor. Restless, active, constantly on the move, Whitney is to be found in all parts of the huge G. E. laboratory. One day he is standing near the lead shield watching Coolidge hurl his cathode rays against a diamond lent by George F. Kunz of Tiffany's. The next he is bending over Langmuir, watching the experiments with gases, optimistic that progress is being made.

Whitney is here, there, everywhere, to the distraction of Miss Christie, his secretary. Sometimes he is down in the basement, where, in electric furnaces, tungsten is being drawn into fine wire. Sometimes he is at the shoulder of a junior member of his staff, a young man working with a microscope. And if he is not in a laboratory he is at his desk talking with some one about the work that is going on. Always he is molding men, always creating and maintaining that atmosphere of harmony and coöperation which makes the G.E. laboratory known throughout the world.

As a result of his personality and his passionate devotion to the cause of research, Whitney is called upon to make innumerable speeches before civic associations and at universities and colleges. He hates having to speak and has expressed the wish that an automatic talking machine might be substituted; one which would make appropriate gestures and speak in clear, fluent, even eloquent tones. But lacking such a machine, he must often yield to the constant pressure, growing stronger every year, despite his protestations that he is a poor speaker. His audiences seem to feel otherwise.

Whitney's addresses reveal many fascinating things concerning his mind. They demonstrate that he is a broadly educated person, that his knowledge is not limited to chemistry and physics. He has pondered the mysteries of anthropology. He finds

gratification in the fact that "the arrival of the vertebrates slowly brought forth brains," that "Herodotus, Socrates, Xenophon, Plato, and Aristotle" were "our first and leading psychological engineers" who started the doctrine of "Know then thyself." He realizes that the miracles of science are giving men time to think. When he thinks of "men" he includes women, too.

"Let her *experiment*," he said in a talk to the girls at Vassar, in touching on the new woman, "and, whether the researches be on dresses or eugenics, in politics or industry, the direction will be right."

It is, he went on, "an interesting young world" we live in, and the miracles of to-day are the commonplaces of to-morrow. The danger is that "we do not appreciate what we so easily discover." At this moment "a man's voice may penetrate to all parts of the earth, but where is the man to say the things worth every one's hearing?" Whitney is, it appears, no narrow specialist in the fields of physics and chemistry; he is a philosopher in the true sense, a man who loves wisdom for its own sake.

It is almost superfluous to point out that the traits and characteristics found in the Whitney of to-day had their beginning in his youth. He has remarked that he is "at a loss to know why so many men go through college keeping their eyes mainly on a ball of some kind when the world is so full of greater interest." From this one concludes, and correctly,

that the youthful Whitney was rated as a serious lad by his companions. He was born in 1868 at Jamestown, N. Y., that city not far from the blue waters of Chautauqua Lake, not far from the fields where sprang up discussion groups at which men talked of religion, morals, and other mysteries and where the Chautauqua Idea had its birth. More than half a century later, ill at ease because he had to make a speech, Whitney stood up to receive the Perkin Medal, a badge of knighthood in American chemistry, for his work in stimulating research. Sir William Perkin, he said, had become a chemist "through the influence of an Englishman named Hall, with whom he came into contact when under fifteen years of age."

"I have the honor," Whitney continued, "to have started as a chemist in this identical manner, and I will tell a little more about it, because I have always wished I had some way of expressing my gratitude to my particular Mr. Hall.

"When I was about fifteen years old a mill owner and one of the leading citizens of my home town, William C. J. Hall, assisted in establishing a Young Men's Christian Association. He had also long been interested in the microscope and was a scientist such as we seldom find among business men to-day. He formed a free evening class for about half a dozen boys—all that could work together around the rotating table on which he placed his immense micro-

scope. This was so arranged that specimen, instrument, and illuminating system did not have to be disturbed as they passed from one boy to another for observation. He did not merely show his specimens, of which he had thousands, but he taught us how to prepare them in all the various ways, now more or less common. They were wonderful to me, and still are.

"My mother gave me some money which, combined with that of one of the other boys, purchased a small microtome, and my father gave me $75 for a microscope. Under Mr. Hall's guidance I bought the instrument, with the understanding that whenever I wanted a better one the old one would be taken back at the original price. I later procured one for $250 which, throughout thirty-five years, I have used almost daily. One of the first experiments I tried with the microscope was to precipitate metallic silver from silver nitrate solution onto a speck of copper filings. Any one who has watched these beautiful crystals grow knows that they are surpassingly wonderful. They constituted my first chemistry. It was those little bottles of salts and bugs in alcohol that led some one to call me a chemist, and it apparently determined my future work.

"It does not seem now as though any one else ever enjoyed a tenth of the pleasures my old microscope introduced to me. I find them inseparably interwoven with about everything I know. Even the bar-

ren North Pole reminds me of Andrée and Amundsen and microscopic algæ which drifted across the polar circle from the Lena delta. The equally barren Sahara reminds me of Darwin and De Vries and the diatoms which were carried by the wind from central Africa and fell on the deck of the *Beagle,* hundreds of miles away."

Whitney began to ride a hobby, evidently, at the age of fifteen. He has been riding one ever since. The nature of the hobby is less important. Whitney is not particular so long as it offers another outlet for his inquiring mind. Once he devoted all of his spare time to looking for Indian arrowheads. He would spend entire days tramping through field and meadow, and when he had found one he would examine it, classify it, learn everything there was to know about it. Then he would compare it with crude tools used by prehistoric men, would dig through musty volumes dealing with anthropology. Another time he became fascinated by turtles and raised quantities of them on his farm. He learned of their private habits, their life-span, their characteristics. Some years ago he was forced to make a trip to the West in connection with the work of his laboratory. On the way out he made several speeches, and on the return trip stopped off to attend scientific meetings. His chief anxiety, however, was to get back home before the turtles came from their places of hibernation, and he was delighted when this turned out to

be possible. He has experimented with numerous agricultural problems, and is actively interested in the application of various physical agencies to the treatment of human ills. He is a student of heredity. And he does all these things, not as a faddist who takes up one subject to drop it for another, but as a serious student. Nothing in nature is alien to him, everything is filled with possibilities that are startling and beautiful. Withal, as we have seen, he is a jovial soul. He has complained, in at least one solemn address, that it is difficult for him to keep from "being facetious." He is said to have dodged several technical powwows to attend a musical show.

Some day a book will have to be written about Whitney, for the many sides of his character cannot adequately be told in a few thousand words. No doubt, the most accurate summation of him would be to say that he feels deeply on all things. No one who did not could say, as Whitney has, that "the only perpetual motion is the growth of truth." Another way to offer a portrait of Whitney's nature is to say that he is fundamentally a teacher. No task is too arduous if through it young men can be taught. He is a member of the board of governors of Union University, a trustee of Union College and of the Albany Medical College, and was for ten years a member of the Corporation of the Massachusetts Institute of Technology. There is slight doubt that he would withdraw, without hesitation, from the

General Electric Company and again go to teaching were he to decide that in such a way the education of young men could better be advanced.

He began as a teacher, in the days before industrial research, as it is known to-day, had been thought of. He had been graduated from M.I.T. in 1890 with the degree of S.B., and was immediately appointed assistant instructor in chemistry. His formal education was, however, far from complete and he spent two years at Leipzig and received the degree of Ph. D. in 1894. He also studied in Paris. It does not seem to have bothered him that progress in an educational institution is slow. He was content to accept, in 1895, the job of instructor at M.I.T. and that of professor in 1901. He has recalled that he once asked for a salary increase of $75 a year and did not receive it, and he thinks that he learned thereby that "financial rewards are not the main thing." His chief recollection of his early days is one of gratitude for the inspiration he received from his teachers. Professor A. A. Noyes, of Tech, he has said, liberated him "forever from the wiles of college football." He had been experimenting with Noyes and had planned to attend a Harvard-Princeton game. The experiment proved so fascinating that he forgot all about the game.

It is incredible, in the light of the position held by scientific research in industry to-day, that it was

almost an unknown thing in 1900. Of course, there were experimental laboratories in some of the larger plants, but these existed chiefly for the purpose of assisting in manufacturing processes. They were "trouble stations," called when something went wrong. What is now known as research was then the unchallenged province of the inventor. It was not until 1904 that Frank B. Jewett, another M.I.T. man, was called to take charge of the research work of the American Telephone and Telegraph Company. Whitney, asked to organize the General Electric laboratories in 1900, has seen research in industry develop before his eyes. And the research laboratory at Schenectady will be, despite his varied work in many fields, his most enduring monument.

Whitney must have known what this laboratory was going to mean. Even in 1900 he looked into the future and saw an industrial laboratory in which pure science would be encouraged. Nothing else would have called him from teaching, a field where young men could be inspired and where, at the same time, truth could be sought for its own sake. Several years before that he had been offered a position by Arthur D. Little, of Cambridge, and had declined saying he would "rather teach than be President." On another occasion he had remarked that unless there were "problems to be solved in Heaven" he did not desire to go there when he died.

Whitney is able always to be attacking problems,

many of them in the field of abstract science, because so many of the everyday details of laboratory operation are handled with efficiency and dispatch by Hawkins, his research engineer. Hawkins is, in many ways, as busy a man as Whitney himself. He must attend to a thousand details, obtain the needful appropriations, adjust complaints, supervise the purchase of equipment and other expenditures, keep things running. He commands the Service of Supply. And yet he has, no less than the director of the laboratory, a clear vision of the true significance of research. Hawkins has said that research is "simply an exploration, a systematized search." It goes on "whether or not what is found is eventually applied." Research is "not the obtaining of additional data regarding what is already known; it is the exploring the unknown for generic facts or principles." It "seeks the new knowledge which makes new things possible."

Such, with a personal leaning toward the acquisition of knowledge for its own sake, was the vision of Whitney when he assumed charge of the new laboratory in 1900. In his direction of the laboratory activities, he is not commercially minded in the sense that he must see an immediate profit from each undertaking. He has the broader vision that sees that the greatest ultimate results come from the scientific pioneering which opens broad new fields of knowledge, but which cannot be expected

to yield immediate financial returns. His point of view was clearly demonstrated when, in the summer of 1909, Dr. Irving Langmuir came to the laboratory.

Langmuir had been teaching chemistry at Stevens Institute and expected to return to his post in the fall. Arriving at Schenectady, he was told by Whitney to look around and determine what interested him most. He noticed that many of the men on the staff were working on the development of drawn tungsten wire under a process devised by Coolidge, and Langmuir suggested that some of the difficulties being experienced might be due to impurities in the wire. There might be, he thought, certain gases present. His work proved so interesting that Langmuir was glad to resign from the Stevens faculty to continue his purely theoretical researches. No definite program was assigned. He was even given assistants to aid in the study of gases in vacuums. At that time all the G.E. experts, including Whitney himself, believed that the way to improve incandescent lamps was to improve the vacuum. But Langmuir continued to play with his gases, merely to see what would happen when he introduced them into the lamps.

"During these first few years," he has recalled, "while I was thus having such a good time satisfying my curiosity and publishing scientific papers on chemical reactions at low pressures, I frequently

wondered whether it was fair that I should spend my whole time in an industrial organization on such purely scientific work, for I confess I didn't see what applications could be made of it, nor did I even have any applications in mind. Several times I talked the matter over with Dr. Whitney, saying I could not tell where this work was going to lead us. He replied that it was not necessary, as far as he was concerned, that it should lead anywhere. He would like to see me continue working along any fundamental lines that would give us more information in regard to the phenomena taking place in incandescent lamps, and that I should feel myself perfectly free to go ahead on any such lines that seemed of interest to me. For nearly three years I worked in this way with several assistants before any real application was made of my work. In adopting this broad-minded attitude Dr. Whitney showed himself to be a real pioneer in the new type of modern industrial research."

In the end, of course, this virtue brought its reward in millions. Langmuir was the first to apply argon and nitrogen to tungsten lamps, and nearly 1,000 million of the new lamps have been made in the United States. The American public has been saved $1,000,000 a night on its lighting bill.

Similarly, in the case of the Langmuir radio power tube, the General Electric laboratory demonstrated that apparently unrelated research may well lead to

a device of outstanding industrial importance. Langmuir, scholar and explorer, studied electron emissions and perfected a power tube capable of amplifying high-frequency current in amounts requisite for radio transmission. If there is a moral in all this it is that if research yields new knowledge in fundamentals, there is no need to worry about its application.

The genius of the gentle and amiable Coolidge is just another example of this. He was studying the phenomena of electron discharges in high vacuum and his new X-ray tube has virtually displaced all other types. It has been developed in sizes up to 250,000 volts and down to a dental tube so small that both it and its transformer can be enclosed in an oil-filled metal casing and swung on a wall-bracket or carried like a hand bag. Now Coolidge is playing with cathode rays developed by giant tubes. No one knows quite where this cathode ray work is leading. No one, if the enthusiastic interest at Schenectady is any indication, worries greatly. The effect of these cathode rays is equal to that of a ton of radium. Its significance is still clouded with doubt, but untold possibilities lie in the new lands which this explorer is beginning to enter. Germs and spores have been killed by an exposure of less than a second. It has been possible to produce the vitamin which cures rickets. The rays have been found to be powerful catalytic agents. It is not impossible that new

forms of energy will be discovered and a new world created. Meanwhile Coolidge is stepping up his voltages, building larger tubes wondering what will happen when he has reached 7,000,000 volts!

But the story of the General Electric laboratory, like that of Whitney himself, needs a volume for its telling. Of all of this work Whitney is the inspiration. He writes papers and makes speeches, sometimes with amazingly revealing titles like "Vacuum, There's Something In It" and "Aristotle was Right." There are at the present time about 400 men and women employed in the laboratory, about half of them scientists. Whitney knows all of them personally, however. When some new man is taken on it is not long before the director has dropped into his laboratory to see how his work is coming on. He is cheerful, encouraging, optimistic. He seems to spread hope and faith wherever he goes. And he knows, in some uncanny fashion, everything that goes on. Whitney is, perhaps, more like Coolidge than Langmuir in temperament. The latter is rather too systematic for Whitney's volatile nature. Langmuir sometimes insists that some particular path is not worth following. Whitney, without thinking about it, sees red when he is told that something cannot be done, and occasionally the laboratory is vastly entertained by a brisk row between the two men. They are, it is needless to say, devoted friends.

Coolidge says, "It's because Dr. Whitney is there

that Langmuir and I can play around. He stands between us and the demands that we do something practical."

It is hard to believe, looking at Whitney, that he is sixty years old. His vigor is that of a man of forty; his curiosity that of a child asking questions about the winds, the rains, and the beams of light that slant down across a room from a window. Of course, he takes pride in the laboratories he has built and the organization he has developed. Naturally he rejoices that it was possible to leave the realm of teaching and find that a great industrial corporation would encourage the search for truth. From time to time another medal is presented to him and the orator of the evening again tells of Whitney's profound knowledge, his lovable traits, his generosity. But Whitney best described himself, unconscious as he may have been of that fact, when Langmuir was the guest of honor and received a medal on a night in March of 1928. On that occasion he said, in part:

"There is something in Langmuir's work that suggests, by sharp contrast, an Oriental crystal gazer seated idly before a transparent globe and trying to read the future without doing anything about it—a hopeless philosophy. In my picture an equally transparent and more vacuous globe takes the place of the conventional crystal sphere. It is a lamp bulb, a real light source. Langmuir boldly takes it in his

hand, not as some apathetic or ascetic yogi, but more like a healthy boy analyzing a new toy, even as Langmuir himself studied and fixed the complex watch of his boyhood days, but seeing visions, too, of many new things. There might have been nothing in that vacuum, but he was driven by insatiable curiosity to investigate and learn for himself.

"Thus he peopled that empty space with new and strange little beings or personalities which he had first dreamed of, then devised, and finally endowed with real character—and all this solely to make his various dreams come true. He first dreamed that tungsten atoms were being carried by foreign atoms (of oxygen) from the filament to the glass to obstruct the light. He dreamed of banishing or imprisoning these, and when he made this dream come true we got good, clear, long-lived tungsten lamps.

"Still Langmuir dreamed, with both eyes on the ball, of a greater light. Therefore he populated the lamp bulb with new beings, rare gases of dependable character, like nitrogen or argon. By this method the light he foresaw in the bulb became just twice as great as it was before, and all of us now easily see it, and the world is glad to pay for it.

"Gazing into the same sphere again, he dreamed about disembodied electricity, and soon the reliable little electrons were tamely obeying special laws—laws that had never been known before and that most men do not yet understand. He gazed again and saw

atoms coöperating with his electrons, so that he was able to add to the thermionic servants of the radio sphere accurately controlled groups of electrical helpers in the shape of gas ions, and thus he continually improved radio tubes.

"Here lies the difference between the ancient and the modern seers or prophets. The modern prophet is a doer. No one can fix the best ratio between thinking and doing. The pure thinker is apt to think too much, and the active man to be too active. Evidently that Mendelian law which determines mutations and produces increased strength by crossbreeding explains why a certain mixture of thinking and acting yields the greatest product. I know of no one who seems to combine these two characteristics in better balanced ratio than Langmuir."

Frank B. Jewett

CHAPTER THREE

PARIS—ON THE PHONE!

Frank B. Jewett

FIFTY-TWO years ago the genius of a single inventor brought into being the telephone. When Alexander Graham Bell called "Mr. Watson, come here, I want you!" the apparatus that he used was the child of his individual creation.

It was, however, the genius of organized research—the group working of many minds—which lay behind experiments which took place in the autumn of 1915. A group of engineers spoke, night after night, into a transmitter at Arlington, Va., in the hope that their voices would speed across 3000 miles of ocean to Paris. There, deep under the base of the Eiffel Tower, another group of scientists waited until, early one morning in October, they heard "and now, Shreeve, good-night."

Watson was an assistant to Bell, and the words which came over a short length of wire in 1876 have assured him immortality. Except for them, he might be unknown to the world at large. Hundreds of scientists had labored to make transatlantic radio

telephony possible and it was merely chance that the first phrase to come through should have mentioned the name of Colonel H. E. Shreeve, now technical representative of the American Telephone and Telegraph Company in Europe. War was sweeping over Europe in 1915 but so important were the radio telephone experiments that the French government permitted the American scientists to listen at the Eiffel Tower at fixed intervals for signals from the United States. Shreeve, the leader of the group, was one of those who tuned in for the ten-minute period granted at 2 A. M. each night. It was to him the engineer at the Arlington radio station happened to be speaking on the morning when, finally, success was achieved. Back of it all were a research laboratory, necessary capital, detailed organization and, in command of the work, General J. J. Carty, chief engineer of the American Telephone and Telegraph Company, and Dr. Frank B. Jewett, who for a decade past had been in charge of much of the important research in telephony. Jewett was directing such researches as led to permalloy, a metal with such remarkable magnetic properties that its use in submarine cables makes possible the swifter transmission of messages. He was responsible for such studies as resulted in virtually perfect long distance telephony. Most important of all, he built up a research organization, now known as the Bell Telephone Laboratories, second to none in the world. Located on West Street

in New York City, it employs 2000 scientists who, aided by more than 2000 assistants, toil daily that man may talk more easily with his fellows, that every form of electrical communication may be improved. It is their dream that neither seas nor mountain ranges, alien languages nor strange customs, static nor storm, shall defeat their purpose.

It took more than fifteen years of patient, careful, enormously costly work for the scientists under Carty and Jewett to develop transatlantic telephony to its present point. On January 7, 1927, commercial service was opened between New York and London, and the broker seeking to close a deal with his English correspondent has but to pick up the receiver of his desk telephone. At first viewed as a novelty, there is to-day more business than the radio circuits available can conveniently handle. The majority of the calls in March, 1928, were from bankers and brokers. But movie actresses telephone from the splendor of Hollywood to their friends in Paris. Theatrical magnates negotiate contracts with foreign stars. Business men, in London on a selling trip, ring up their wives. Débutantes telephone their fathers for additional funds for Paris shopping. The average number of calls per day during 1927 was 7.10. For the week ending April 21, 1928, the average calls per day were 46.20. Communication has now been established not only with London and Paris but with Germany, Holland, Belgium, and

Sweden, and new radio circuits will be opened in the near future. The patient efforts of men who deal with the minutiæ of science are again returning a harvest of wealth.

"We of the Bell System," said Jewett in an address several years ago, "have taken pride in acknowledging our indebtedness to science and in pointing out the many achievements in electrical communication where noteworthy results of a most practical nature have direct ancestry in the solution of some problem of physics or chemistry undertaken without thought of utilitarian use but merely in the quest of a broader understanding of nature."

To as great a degree as any other laboratory in the country, the Bell Telephone Laboratories represent the new group idea in research. The development of long distance telephony, apparatus for reproducing photographs by telephone, the prospects of television; all these were the fruits of combined effort. In all such organizations there is, however, a directing head, and much as Jewett may object to that rôle, he is the one who must take the bows. As vice president of the American Telephone and Telegraph Company and president of the Bell Telephone Laboratories, he is the responsible high official. It is he who must make the decisions where large expenditures, fundamental research problems or matters of policy are involved.

Jewett has his office on the twenty-sixth floor of the

Telephone and Telegraph Building at 195 Broadway, looking towards the north over the city. His environment is about as different from that of the average research worker as it could possibly be. He no longer hears, at least not just outside his office door, the hum of dynamos and motors. But this is due to circumstances, not desire. A combination of research worker and teacher, he developed talents as an executive. During the twenty-odd years that he had been in charge of laboratory activities, his main task had been to create the proper atmosphere of coöperation. He appreciated, too, the vast gap which lies between the scientist in industry and the financier, lawyer or capitalist whose type is in the majority on every board of directors. The scientist, appearing before laymen, finds himself at a loss for words which can be understood. The nontechnically trained business man, depressed possibly by a sense of inferiority, is inclined to scoff at the apparently academic theories likely to be advanced. Jewett knew himself well enough to be aware that he could serve in a liaison capacity. So he turned, reluctantly, from his love of research and became an executive. In doing so, he has advanced the cause of research, the welfare of the men engaged in it, and the prosperity of the telephone industry.

There is nothing very colorful about Jewett's personality. Men who have known him for twenty years, and who have worked and played with him,

scratch their heads and admit that they cannot recall a single anecdote concerning his life. Jewett is enormously capable, they say. He is efficient, logical, a true scientist. He enjoys boating and knows every rock and tree on Martha's Vineyard, where he spends his summer holidays. He keeps up with the latest books. He shaves with an old-fashioned straight razor, whatever this conservatism may be worth as a clue to his character. He carries in his pocket a folding rule, exactly one foot long, as other men carry fountain pens. An object is not "about a foot in length" to Jewett—it is between eleven and one quarter inches and eleven and five-sixteenths inches. This ruler he carries is an excellent clue to his precision and his exact mental processes.

Jewett does too much work and permits himself to be dragged into far too many things. He is approachable and human, but in a rather reserved way. An office boy who has worked for him a decade before can always obtain an interview and will find Jewett sincerely interested in the progress he has made. Education and its advancement are two of his chief interests. Except for boating, his garden, and a mild flair for Phœnician glass, he has virtually no hobbies.

The visitor calling upon Jewett finds a slender, dark-haired, slightly bald, scrupulously dressed man standing behind a desk placed in one corner of

his large office. It is not hard to see why Jewett is successful in his difficult rôle of interpreting science to men trained in finance. He talks well, rather slowly, with assurance and precision. He seems to establish a contact quickly, to be sure of his ground. As an interview begins, Jewett sits in a fairly erect position. Sometimes he will say hardly a word during the first five or ten minutes; the caller is permitted to state his proposition in his own way. All the while Jewett is slumping lower in his chair. He is likely to swing in a half-circle and place his feet on the low radiator in front of the window. He is not addicted to push buttons. There are no interruptions, the business at hand appears to be his only thought, and secretaries do not dash in and out with memoranda, papers to be signed, announcements that somebody is on his telephone. Finally, as the visitor has finished, Jewett has slouched down until the back of his neck rests on the back of his chair. He pulls himself up, ever so slightly, looks out of the window.

"Well," he says, "as I see it, the problem breaks itself down into these elements——."

Nothing is more typical of Jewett than this tendency to resolve a problem into its logical parts. He proceeds, at least in his mind even if he does not voice it, with a "firstly" and concludes with a "lastly." Like Herbert Hoover, he exudes facts. But he has less ability than Hoover in turning

a pretty phrase. He exudes facts and yet is almost philosophical when he talks about education. It is significant of him that he believes errors practically impossible as long as all the facts are at hand. This causes him to scan every project with the utmost care and to look askance at any enterprise in which success is not virtually assured. Once involved, he gives in full measure his time and his intelligence.

In examining the histories of American leaders of research, and the same is undoubtedly true in other countries, the observer is struck by one frequent occurrence—that all these men arrived at positions of importance early in life. It is also usually true that they were men who achieved scholastic honors in college. The embryo scientist, it would seem, has better use for his time than the football field or the cinder path. He has much to learn and the need for concentrated work is great. Jewett was no exception to this general rule. He was an excellent scholar and soon after he passed his twenty-fifth birthday he was in charge of the engineering research work of the American Telephone and Telegraph Company. When he was thirty years old he was transmission and protection engineer and worked under the direct supervision of General Carty.

He feels that it was of no significance that, as a small boy, he had a mechanical bent. Perhaps this is because, to-day, he is besieged by proud parents,

whose sons have built a radio set, for advice regarding the education and training of youthful mechanical geniuses. But Jewett's technical skill was, no doubt, partly inherited. He was born at Pasadena, Calif., on September 5, 1879, the son of a civil engineer and the builder of what is now the California division of the Santa Fé Railroad. It was agreed that he should enter some form of engineering, and since hydroelectric developments were beginning to take shape in California, this branch seemed most attractive. He attended Throop Polytechnic Institute, now the California Institute of Technology, and was graduated in 1898 with the degree of bachelor of arts. But he had only started his education. Some friends had gone East to the University of Chicago, and Jewett followed them to specialize in physics, mathematics and chemistry. In 1902, twenty-three years old and after having been assistant to Professor Michelson, he received the degree of Doctor of Philosophy. All this work was somewhat remote from practical electrical engineering, which he had studied in California. So he went farther East, this time to Massachusetts Institute of Technology, where so many incipient directors of research have received training. Jewett had hoped there to find graduate work in his field. But the courses offered were not what he wanted and he drifted into teaching.

Life in Massachusetts proved attractive to him,

understandable enough in view of the fact that he is of New England ancestry. His forebears landed at Rowley, Mass., in 1632 and among the early Jewetts were men active in civil, social, and religious life in the colonies. The family scattered with the years, pushing out toward the Middle West for more room in which to breathe. But some remained behind and these continued their interest in the intellectual aspects of developing civilization. Jewett's great-uncle and his grandfather were the first publishers of Harriet Beecher Stowe's *Uncle Tom's Cabin.* Another great-uncle served for many years as librarian of the Boston Public Library—this in the days before Boston became excessively timid about literature and proscribed two or three books out of every dozen.

Having arrived at Tech, Jewett was asked to teach some courses and he did this until 1904, engaging in electrical research at the same time. The headquarters of the American Telephone and Telegraph Company were still in Boston, due to the fact that Bell's laboratory had been in that city and so the forerunner of the parent Bell company. Officials of the company were in close touch with the electrical experts across the river at M. I. T. Jewett came into contact with them, Carty among others, and in 1904 he entered the engineering department of the company. He had been interested in the science of telephony for some years and was confident that it

offered unusual possibilities for research. Soon after he joined the organization he was given charge of the research work. From 1908 to 1912 he was transmission and protection engineer and then, under Carty, he worked out various of the technical improvements. In 1912 he became assistant chief engineer of the Western Electric Company, the manufacturing branch of the industry, and in 1916 its chief engineer. During the war, as a major in the Signal Corps and later as lieutenant colonel, he assisted in the recruiting of men from the Bell System and served on a special naval board attempting to solve submarine detection problems. In 1925 the research activities of the Western Electric Company and the American Telephone and Telegraph Company were merged into a new corporate entity, the Bell Telephone Laboratories, Inc., and Jewett became president. He was at the same time made a vice president of the parent company.

The pioneers of telephony were men with a breadth of vision seldom found among those who start a new industry. Back in 1885, when incorporation papers for the American Telephone and Telegraph Company were filed, the telephone was still a curiosity to most people. The transmission of the human voice was far from perfect, and Bell had been considered guilty of rank overstatement when he had predicted that, in due time, men would speak from one city to another. The incorporation papers of the American

Telephone and Telegraph Company authorized the company to develop telephone and telegraph lines between New York City and all cities in New York State, various cities on the continent, and "by cable or other known means with every part of the known world." It was merely a dream in 1885 but to-day, as Jewett has said, "the voice can be carried over practically any terrestrial distance."

Obviously, it is impossible to recount all of the advances which have been made in the telephone art in the past two decades. If to-day there are no terrestrial limitations it is because men worked patiently in laboratories, often on matters that seemed to have no bearing upon telephony itself. First came the mechanical repeaters or amplifiers which brought perfection in long distance communication. More than fifteen years ago scientists working under Carty and Jewett began the experiments which led to the present transatlantic service. The transatlantic service is not solely radio; it is a combination of aerial and land telephony. For a number of years before work started actively on radio telephony, it was appreciated that a transoceanic system, to be of value, must be developed in connection with long distance lines. The problems to be solved were technical, scientific, engineering, economic, and social. The solution to most of them could not have been found had it not been for the research organization already built up. Here were metallurgists, experts in magnetism, in

radio, in practical telephony. On the staff were men who could negotiate, with all the finesse of diplomats, arrangements with foreign governments.

Since transatlantic telephony depends partly on long distance land transmission, this had to be perfected first. It had been done by the end of 1914; New York and San Francisco were connected by three circuits, each using a different form of amplifier or mechanical device to magnify the voice impulses. The most successful line was that on which the thermionic vacuum tube, invented by De Forrest in 1906, was used. This type of repeater has since become the basis of nearly all long distance work and it made communication across the Atlantic possible. Laboratory experiments demonstrated, at about the same time, that many of the obstacles in the path of commercial service were mechanical in their nature, and work in the field began. Jewett and Carty were in charge of the developments. It was Jewett's duty to decide large matters of research policy, such as whether experiments were to continue on a short or long wave basis. As Jewett puts it, he had to decide whether "to place millions on this chip or that." But he made his decision favoring a long wave, as he always does, with the guidance of a vast accumulation of facts and all his assistants were in agreement with him.

The field tests began in 1915. Montauk Point, on the far tip of Long Island, was selected for the

first transmitting station and the top of a tall building in Wilmington, Del., for the receiving station. The sending station was not particularly powerful, but sentences read into the transmitter were distinctly heard in Wilmington. Then a land wire was run to Montauk from Wilmington and the voices received through the air relayed back along it. Thus those who spoke could hear their own voices and it was demonstrated that aerial and land telephony could be used in combination. Jewett and Carty next decided to lengthen the distances; a receiving station was established in Georgia, 800 miles from Montauk Point, and again the experiments were successful. It was now agreed that transoceanic tests could be made.

There were many problems still to be solved. Powerful vacuum tubes had to be developed. While this work was being carried on at the Bell Telephone Laboratories in New York, engineers with receiving sets were sent to the Isthmus of Panama, to San Francisco, to the Hawaiian Islands, and to Paris. This may have appeared extreme optimism, inasmuch as the necessary tubes had not yet been made. But that it was justified was shown when, soon after the engineers had departed, a 25-watt tube came from the laboratories. This time the naval radio station at Arlington, across the Potomac from Washington, D. C., was used to send the voices to the four points where the engineers waited. Panama was the first

station to report success. Then San Francisco announced that the voices had come through. And in September of the same year, 1915, Theodore N. Vail, then president of the American Telephone and Telegraph Company, spoke from New York to Carty in San Francisco. His voice went by wire from New York to Washington, and thence by radio to the Pacific Coast. Carty answered by long distance telephone. Subsequently even Honolulu reported that the voices from Washington could be heard, and this although the distance was more than 5000 miles.

The engineers at the Eiffel Tower were having infinite trouble. The French authorities had given them the right to listen for signals at 2 A.M. each day but Europe was a network of high-power stations filling the air with military messages. On a night in October the signals came through, however. Some one at Arlington had been talking into the transmitter for several minutes, according to the nightly custom, and his words finally pierced the fog of other radio stations and static. Shreeve, who chanced to be listening, heard, "and now, Shreeve, good-night." On several following evenings reception was clear and unmistakable.

Transoceanic radio telephony was an experimental success, but the entrance of the United States into the World War forced Jewett and his associates to give all of their time to war work. Transatlantic

radio was, for the moment, forgotten while they worked with army officers on communication between airplanes and artillery batteries and on many other problems. As soon as the war had ended, the trans-oceanic experiments were continued, now with definite knowledge that success lay ahead. Arrangements were made with the Post Office Department, which handles telephone and telegraph communications in Great Britain, and in January, 1927, service between New York and London was opened to the public. A few months later it was extended from all of the United States and Cuba to all of Great Britain. To-day, with the addition of Canada and Mexico, France, Belgium, The Netherlands, Germany, and Sweden, it is estimated that the 120,000,000 English-speaking people on the North American continent can communicate with the 60,000,000 inhabitants of European countries thus far linked into the system. This is a thought which profoundly impresses Jewett. He sees in it the hope of better understanding among nations.

"The use of the spoken word to convey ideas," once said Carty, "distinguishes man from all other created things. It is the function of the engineer to provide for the extension of the spoken word by means of electrical systems of intercommunication which will serve to connect the nervous system of each unit of society with all of the others, thus providing an indispensable element in the structure of

that inconceivably great and powerful organism which it is believed will be the ultimate outcome of the marvelous evolution which society is to undergo."

The lay mind finds it difficult, almost impossible, fully to appreciate the details of this miracle of research. It is all, however, part of the day's business down at 24 Walker Street in New York City where the transatlantic "board" is located and where picked girl operators, chosen from among all of the thousands in the employ of the company, work with technical experts to make the service a success. In charge of Miss Ella Mae Higgins, the girls work with precision and accuracy. All of them speak at least one language besides English. All have made records for efficiency. The circuit is now open from 6:30 o'clock in the morning, New York time, until 9 o'clock at night. The corresponding hours in Great Britain are 11:30 A.M. to 2 A.M. The girls in charge of transatlantic calls sit in front of switchboards which look very much like those in any central office. When the service was first opened they would gossip, during lulls, with the English operators. Now, however, it is all very much part of the day's work and Mary in England merely remarks "good morning" to Velma in America as the morning work begins.

Despite all precautions, static occasionally interrupts the service. Without warning, it makes successful transmission impossible. Under ordinary cir-

cumstances, however, a conversation between Europe and America is as distinct and clear as one between two people residing a block away from each other. This is so because the company experts are constantly on guard. Unlike a land circuit, the radio link in the transatlantic circuit is subject to changes throughout the day. The technical staff must, therefore, keep the speech volume constant and must attempt to counteract any atmospheric disturbance. The transmitting station in this country is at Rocky Point, L. I. The voice of the subscriber speaking from his desk telephone (which may be located in San Francisco) is carried to the Rocky Point sending station by long distance telephone via Walker Street switchboard. Then it is amplified and sent hurtling across the ocean to Cupar, Scotland. At Cupar the voice is again amplified and transmitted to London by long distance wires. All calls destined for Europe pass through London and by the ordinary telephone cables to the continent. The returning voice goes by land wire to Rugby, England, where it is amplified millions of times and sent by radio to the American receiving station at Houlton, Me., a northerly point where static is likely to be least disturbing. There the voice impulses are transmitted, after amplification, by land wire to 24 Walker Street in New York. Of these complex routings and processes is the transoceanic connection made.

"Visitors seem to find one feature of our work little

short of marvelous," explains Nat D. Wells, chief technical operator at Walker Street. "Because the radio channels carrying a transatlantic conversation back and forth and both east and westbound use the same wave length (5000 meters) some of the voice sent out from Rocky Point is picked up by the station at Houlton. Thus it is necessary to cut out the channel used by a British speaker when an American is talking and vice versa. This is done by short-circuiting the channel not wanted during the utterance of a particular word. The sounding of part of a word automatically opens and shuts a series of voice relays, swiftly opening the channel when it is needed and closing it when it is not needed."

From a research point of view, transatlantic telephony has reached a more or less completed stage, and Jewett has turned much of his attention to other things. He is frequently annoyed by rash newspaper predictions as to what science is likely to accomplish and takes issue with those who, for example, predict television in every home in a few years. Overoptimistic predictions, he is convinced, undermine the public confidence in research. On the other hand, he feels that the increased interest in the achievements of research workers is causing thousands of young men to ponder science as a career.

"The scientist," he has said, "now holds a more dignified position in the public eye. This, I am sure, has much to do with the fact that young men are

turning to it. Toward the end of the Middle Ages there was a great movement toward art, inspired partly by Michelangelo. Not many were good artists, and few were qualified. But it was the respected thing. In our own country all the best young men once went into business, into railroading, into the building of personal fortunes. It seems to me that there is a discernible shift away from this and science will benefit thereby. The young man who pursues a scientific career does not do so in the belief that he can make a great deal of money. He wants, of course, an adequate income and he is entitled to it. But the motivation is curiosity about things yet unknown.

"There are two phases to the problem of obtaining enough research workers, maintaining the proper standards, and enabling students to continue their research work. The first depends upon good teachers. The second can be achieved through such endowment funds as the one now being raised in the National Academy of Sciences with Herbert Hoover as Chairman of the Board of Trustees. I think the universities are doing their part.

"A good deal of nonsense is uttered, I think, about the functions of industrial research. It is not 'pure science'; it seeks a definite objective. Pure science should be left to the universities; particularly is this the case with public utility corporations whose expenditures are regulated by governments. Obviously,

we cannot support men to work on, say, botanical problems which have no conceivable relation to the telephone business. We have no warrant for taxing telephone subscribers for such work. The work being carried on must have at least a remote relation to the business of the company.

"And yet this does not mean that the research must bring guaranteed cash returns. It may appear that the man is working far afield when, in fact, he is running down fundamental knowledge. The picture I often give is of two men climbing a tree. For a time they go up the trunk together. The pure science man goes out, let us say, on the first branch. It is just as interesting as any other and he looks carefully at the twigs and the leaves. The industrial scientist may start out on the same branch. But if it bears no relation to his main problem he will return to the trunk and find another branch, no matter how alluring the first may appear.

"At the present time close coöperation exists between the universities and the industrial laboratories. Some years ago, in fact, the scientific schools were inclined to ask us to arrange their courses. Obviously, this was a mistake. We could tell them only of the present state of the telephone art. Any courses suggested by us might be out of date by the time the student graduated, for the science of telephony is like other sciences in that progress is continually being made. It is the task of the university to sift

the raw material that comes to it, to encourage the talented student, to drive home the fundamentals, to create the proper outlook toward the field of science."

Jewett, like C. E. K. Mees of the Eastman Kodak Company, believes that the large industrial research organization must make a place for the individual genius, the man with an inventive turn of mind. The modern laboratory, he thinks, makes it possible for such individuals to work in harmony with other men. No longer do they work in secret and starve in garrets. On the contrary, they command the best equipment that money can buy. Often, Jewett concedes, inventors are difficult to control, unsocial in their outlook. But gradually, as they become accustomed to the new order, they learn to confer with their associates. The day when every laboratory worker was suspicious of his fellows has passed forever, Jewett is confident. The inventive genius, he says, is welcomed at the Bell Telephone Laboratories.

Such is Jewett, a man of two sides. On the one, he is the competent executive, efficient to the last degree, a reserved individual in outward manner but with an undernote of kindliness that really is closest to his real character. On the other, he is the scientist, passionately interested in the education of young men, willing to give of his time and his energies that the laboratory of to-morrow may be adequately staffed. There is, too, a third side. Jewett is what

men call "a good citizen." He lives at Brantwood in Short Hills, N. J., and his neighbors know that he can always be relied upon to lead in any movement for the betterment of the town. In this, as in his devotion to education, he overtaxes his strength.

Occasionally there is one man in an industry (or in a community) whose integrity is beyond question. When some bitter quarrel is going on he is called in to adjust the dispute. Beyond any other he is trusted by both sides. Such a man is Jewett. He is frequently asked to arbitrate and his decisions are accepted without question. People seem to realize that Jewett makes no decision until all the facts are before him, and he convinces them that, with all the information in, there can be no wrong finding.

CHAPTER FOUR

BRIGHTER THAN THE SUN

Elmer A. Sperry

ALMOST sixty years ago, in the little village of Cortland in the central part of New York State, lived a youth with an agile, inquiring mind. The age of machinery was dawning, and Elmer Ambrose Sperry cheerfully sacrificed food or sleep or recreation for an opportunity to learn about the operation of some new piece of apparatus. Then, as soon as he had mastered it, he began to ponder some way of improving upon it; always confident that he could do it without difficulty. The rickety, ungainly locomotives that puffed and wheezed along the tracks on the outskirts of the town were his particular fascination, and at the age of fourteen he had devised a swiveling headlight to give illumination around a curve. He prepared a complete set of drawings for this and hung around the station in an attempt to convince the engineers and firemen that his idea would make railroading vastly safer.

The youthful Sperry experimented in the barn at his home with flywheels and with models for wind-

Elmer A. Sperry.

mills to be set up on neighbors' barns and guaranteed to do the churning or to amuse the baby by agitating the arms of a jumping jack. He built a railroad tricycle by means of which he kept ahead of the local freights and drove the engineers to new outbursts of profanity. He astonished his companions by performances with a glass-blowing outfit, slightly injured one of them during a demonstration of the usually self-evident truth that benzine vapor explodes, and built water wheels in the streams that flowed down from the hillsides. The aunt who took care of him—his mother having died when he was quite small—watched him with a mixture of amusement and awe and called him, during less irritated moments, "Aunty's Old Fusser."

Sperry has been fussing with something ever since and to-day there is hardly a ship on the seas or an airplane in the sky which does not sail or fly more safely because of his innate certainty that he can improve almost anything. His gyro-compass and mechanical helmsman to which he refers familiarly either as "Metal Mike" or simply "he," has been installed on practically all great ships throughout the world and insures, with the assistance of nature herself, a straight course to port. His new searchlight, known technically as a "high intensity arc" because a temperature of 5000 degrees can be achieved at the fierce crater of the carbon electrode, gives a beam that might be seen for 1000 miles were

it not for the curvature of the earth. It has created light comparable to that of a billion candles and not long ago, down at a flying field in Virginia, an aviator took off in its beam with a newspaper propped in front of him. At twenty miles he could read it as easily as though the sun were high in the heavens. At forty miles the light had diminished but little. The type was still legible at fifty miles from the field. The new Sperry searchlight provides a light 700 per cent greater than any other practical one ever achieved by man. An astronomer working on Mt. Wilson found that the intensity of the arc transcended that of the sun itself; that the sun's rays reached the earth in the proportion of 920 candlepower per square millimeter, while the Sperry searchlight created from 1140 to 1200 per square millimeter. The Sperry searchlights guided the first air mail on its perilous journey across the mountains and deserts of a continent. They poked their inquisitive rays through the fogs above London and enabled the anti-aircraft defense to combat the Zeppelins. They are used on virtually all the vessels of the United States Navy, and no flying field is considered adequately equipped without at least one of them.

Now sixty-eight years old, Sperry sits in his office on the eleventh floor of a large building located at the Brooklyn end of the Manhattan Bridge, overlooking that inspiring skyline of lower New York,

and tries to choose among the many growing children in his research kindergarten the one of their number making the lustiest demand for his ministrations. He says that the little he has saved from each of the numerous forerunners of "Metal Mike" has aided him to build the Sperry building. In it are manufactured some of the many devices that are the fruit of his creative and exploring mind. In these, except in the degree to which he had a part in giving them life, he is no longer deeply interested. He seems to care nothing whatever about the business details of the large corporations built around them. He is looking for something new.

Seated behind his desk, Sperry looks like a tall man, an illusion dispelled when he arises, for he is of only medium height. But he is decidedly distinguished in appearance; the type of man pictured by every automobile advertiser as favoring his particular car above all others, the type seen lunching at the Bankers' Club on top of the Equitable Building at noon. White-haired, with a white mustache, scrupulously tailored, Sperry looks very much like the traditional picture of a bank president. His eyes, however, light up more easily. They are similarly keen, but they reflect more enthusiasm than those of a man accustomed to turning down loans. He looks like a banker, then, but he talks like a salesman—freely, with vigor, without technical obscurities and at great length; usually contin-

uing until his secretary interrupts to say that he is already a half hour behind his list of appointments.

Sperry has no false modesty, nor is he diffident about discussing the children of his brain. But any impression that he is conceited is dispelled by an impersonal note which surrounds what he says. He seems to feel, in fact, that the wierd and wonderful devices of his creation are not children in any sense, but adults. They are doing useful work in the world, even brilliant work. They make ships safe; they enable aviators to wing their way home, like pigeons.

"See?" he will demand, after explaining some mechanism. "Isn't that wonderful?" referring always to the working of nature's laws—which interest him intensely—that the operation of that particular mechanism exhibits.

One of the most interesting aspects of Sperry is the singular way in which he talks about his work. He is impersonal only in the sense that Sperry, to whom the world of science gives full credit, seems to be furthest from his thoughts. His inventions, far from being impersonal, take on human attributes as he speaks. It is a little uncanny to sit across a table from him and listen to him holding forth on the gyroscope. The mechanism becomes alive. It becomes "he" or "that brute"—as he recalls early difficulties—or "Metal Mike." It is possessed, he makes clear, of weird reasoning powers.

"Why, if he were sitting in that chair,"—Sperry

points to a chair beside him as he speaks, and the uninitiated visitor glances at it a trifle uneasily—"if he were there, he could tell us all sorts of things! He knows that the chair is moving, and its velocity and its direction. He can feel motion about an axis actually 4000 miles away. He knows the exact relation of the axis of the earth to this room. You and I have no such knowledge, but he knows intuitively. Isn't that wonderful?"

One is puzzled, in surveying the life of Elmer A. Sperry, to know whether he is fundamentally an inventor or a research worker. Thomas A. Edison is widely conceded to be the last of the old-style inventors; a man who played a lone hand, who charted unsailed seas without a crew, whose miracles were the result of his own groping and flashes of genius. Is not Sperry, with 400 patents covering a multitude of fields, the same type? Asked this direct question, Sperry prefers to be known as a research worker and such he probably is.

"The inventor worked alone, blindly," he says. "He did not know what had gone before. He was usually poor, unable to obtain apparatus that he needed, handicapped by not having enough to eat. He would often work for years, wasting half of his life, before he perfected his device, only to find that some other inventor had been at the patent office ahead of him. The history of invention is filled with heartbreaks.

"All this has now changed. The research man is assisted by modern system. In the industrial laboratory his apparatus has been purchased by a corporation well able to afford the best of everything. Patent attorneys are at the elbow of the research worker and inform him, before he starts, what has been done in the field. They watch developments at the patent office and tell him when anything relating to his experiments is reported. All literature, scientific and otherwise, having any relation to the subject is in his laboratory library. There is no waste, no lost motion, no possibility that years of labor will be wasted because of a lack of knowledge."

But Sperry is a research worker by evolution from inventor. He began in 1879, then only nineteen years old, by perfecting a device which made the first arc light profitable. The next year he founded the Sperry Electric Company of Chicago and manufactured lamps, dynamos, motors, and other electrical appliances. A few years later he was inventing mining machinery, designing trolleys, electric automobiles, a process for producing caustic soda, a detinning process, fuse wires. So it has been throughout his life, as the patents on file in his name testify. So it is to-day.

It is not, perhaps, inaccurate to say that Sperry has adopted the research method chiefly for his associates and is a member of the new order on that basis. He works, himself, about as he always has.

He uses his extraordinary knowledge of chemistry, physics, electricity, and engineering just as he has used it in the past; feeling his way, trying this scheme and then that, wondering whether there is not a short cut to his objective, quite convinced that there may be something in his long experience which will enable him to pit his own innate knowledge against the experiments of his associates.

A visit to the Sperry Building in Brooklyn, combined with a few hours of informal conversation with the men bending over complicated pieces of apparatus, confirms the impression that Sperry has changed only superficially with the years. He is, to borrow a term from current baseball, the "master mind" of his team. John McGraw of the New York Giants, as those who follow baseball are aware, is widely known because of his ability to sit on the bench and run his team. McGraw nods, ever so slightly, and the pitcher sends a low, fast ball to the plate. He wiggles an ear, and the runner on second steals to third. Sperry's similar talent, in the complicated realms of engineering and science, is little short of astonishing. And it fills the engineers, chemists, and physicists who are his subordinates with amazement, awe, and—not infrequently—discreet merriment.

To picture Sperry in the process of development and research investigation one must return, again, to his offices on the eleventh floor of the Sperry Build-

ing. As soon as he arrives in the morning his secretary begins to summon the various engineers. Sperry is no longer strong enough to visit constantly the laboratories and workrooms as was his life habit, and the men in direct charge are called to his office. So begins the day. The first expert to arrive is working, perhaps, on the Diesel engine. Sperry is deeply interested in Diesel engine development and believes that it can be made light enough in weight to be used for airplanes. His design will practically eliminate the fire hazard in airplane crashes and use a less amount of a far cheaper fuel for a given flight. The chances are that he has not seen the engine under development for days or even weeks, but this fact does not disturb him. He grasps the lines of a blueprint like a dressmaker looking at a paper pattern. He is visually minded to an unusual degree and at his desk he can picture the engine with all its complications of valves and cylinders. The engineer in charge describes such progress as may have been made. Sperry listens for a moment or two and interrupts with a question. Then he makes a suggestion, drawing swift diagrams on a paper in front of him. Sometimes he master-minds so swiftly that he is far ahead of his associate before the interview is well under way. The engineer stumbles out, at its conclusion, feeling slightly groggy.

Obviously, Sperry has an intuitive mind. An engineer, he has no trace of what has been called "the

engineer's horror of a hunch." As though equipped with a sixth sense, he describes the exact manner in which some bracket for a searchlight should be designed—so many inches at this point, so thick at that; and when the experts in the laboratories downstairs are finished with their mathematical calculations they find, to their chagrin, that the "Old Man" has figured tensile strength to a decimal. This is the result of years experience.

That Sperry is by nature an inventor, and only by cultivation a research worker, is understandable in the light of his boyhood and education. He was born in 1860 in Cortland, N. Y., of excellent New England stock. His mother was a woman of culture very unusual for her day; she had studied higher mathematics and excelled in it. The greatest tragedy of Sperry's life, except perhaps when his aviator son, Lawrence Sperry, was lost in the English Channel, occurred when his mother died soon after he was born. She was but twenty-one years old. But she left behind textbooks and other volumes on mathematics and astronomy, with notes in her own writing on the margins of the pages. These became an inspiration to the boy.

There seems to have been no poverty or real hardship during his boyhood. Cortland, on the edge of the Finger Lake country in central New York, was a beautiful place. In the winter the snow piled high, but during the summer there were dashing mill

streams and green fields and paths that led through the woods. Sperry's father, his paternal grandparents, and an aunt did what they could to make up for the loss of his mother and they succeeded so well that the mother became, no doubt, something of a misty legend to the son, a legend made real only by the books she had left behind. Cortland was no backwoods town with a single red schoolhouse waging a losing fight against illiteracy. In the early seventies it was known as a minor seat of learning, and later for one of the first large state normal schools. It was, with Cortland Academy, a Mecca for country boys and girls who wished to train their minds and become teachers. A few miles distant, atop a hillside that looked down over the shifting gray and blue and green waters of Cayuga Lake, was the new Cornell University where, so its founder had decreed, "any person" was to "find instruction in any study."

Sperry attended the classes at the state normal and did very well indeed, particularly in mathematics. Even in the primary grades he was fascinated by an instrument case connected with the physics department and made friends with the instructors. And the great moment of his boyhood came when he was sixteen and funds were provided for a visit to the Centennial Exposition at Philadelphia. There he haunted the machinery building and stood, awed and silent, before the great Corliss engine with its stupendous flywheel. On one occasion he was actually

permitted to grasp the starting wheel as the engine was set in motion. He returned to Cortland dreamy-eyed and filled with visions of what, some day, he would do himself. He turned with new frenzy to his water wheels, his windmills, his experiments in the barn.

While still at the Normal he managed to get over to Ithaca on short trips and scraped up an acquaintance with the physicist, William A. Anthony, and the engineer, John A. Sweet. The hydraulic laboratory at the foot of Beebe Lake, now an established sight from the bridge that spans the gorge beneath, was then being constructed. Sperry, who knew something about hydraulics, became an unofficial supervisor. He rode up the hill from the city on the trucks that carried the machinery, watched its installation, understood its mysteries.

In 1879 he entered Cornell, but the urge for invention being already strong, he left after a single year. He had, in fact, already made a name for himself as a kind of eccentric genius, and several men in Cortland agreed to back him in manufacturing his arc lamp and dynamo. Additional capital was offered soon after he placed his first one on exhibition and he moved to Syracuse where he built a larger lamp and dynamo. Success came swiftly, so swiftly that it would have ruined most young men. By the time he was twenty Sperry was the proud owner of a factory in Chicago. He had gone

West because greater opportunities were offered and he has never ceased to congratulate himself for doing so. To this day he believes the Middle West to be the real heart of America and he is confident that only there do people think clearly and constructively. The East, he feels, has been contaminated by immigration. For instance, he is puzzled by the distaste for prohibition in New York. He neither drinks nor smokes, himself, and he is sure that the Middle West, consistently voting for dry laws, is truly typical of fundamental Americanism.

It is impossible to dwell at great length on the astonishing rise of the youth who had become a successful inventor—that rarest of all phenomena—at the age of nineteen. So varied were his inventions that page after page could be devoted to their discussion. For those who seek a detailed story there is a pamphlet, published in 1927 when he was awarded the John Fritz Medal "for notable scientific or industrial achievement." This pamphlet describes most of the Sperry inventions and their technical significance. It contains, too, an excellent biographical sketch written by Gano Dunn, past president of the American Institute of Electrical Engineers.

Commercial enterprises have never appealed to Speery, and it is interesting to notice that as soon as one of his inventions was being manufactured in quantity he lost interest. As soon as he could do so,

he turned the management over to others. Sometimes he sold out his interests. Often he was content with a small royalty. To-day companies are operating on the basis of his patents in all parts of the world; and of many of them Sperry has not the slightest knowledge. Their output has run into the millions each year. We note, however, that his first love, the arc light, is still close to his heart. With the Diesel Engine, the gyro-compass, the gyro-ship stabilizer, a device which detects fissures in steel rails, and a few others, the searchlight now occupies a large part of his time. And it is interesting, too, that in perfecting his arc Sperry adopted, for one of the few times in his life, the orderly and routine methods of the research worker.

In 1883 Sperry erected, on the shore of Lake Michigan at Chicago, a beacon which was the tallest and brightest in the world. A total of 40,000 candlepower was achieved through a group of arc lights. From that day, until Sperry announced his new high-intensity arc searchlight, about eleven years ago, there had been no change in the established type of carbon lamp. Every one thought that it had reached its ultimate development, and illuminating engineers were turning to incandescent bulbs, with reflectors, and other devices. But Sperry began to ponder the matter. What limited the brilliance of the arc light, he asked himself? Would additional

current bring more light? Might not something besides carbon be used for the electrodes?

"It was," he now recalls, "a very pretty problem to fool with. There was all the science in it that any one could ask."

In 1914 the United States Navy sent officers to Sperry to tell him that new searchlights were badly needed. The range of guns had increased. The present lights revealed the position of a battleship without enabling it so much as to see the enemy. Could Sperry make improvements? Sperry had probably not thought of arcs and their technicalities for twenty years, and his mind went back to the early experiments of his youth. He knew that the light in an arc lamp was caused by the incandescense from the positive crater of the pure carbon arc. Carbon resists the flow of electricity. The resistance causes heat and then volatilizing. He found that the positive carbon could be heated to 3700 degrees and that at this temperature 160 candlepower per square millimeter was achieved. Additional electrical current did not increase the specific light intensity, but merely the rapidity of the volatilizing of the carbon. All electrical engineers were aware of this and had assumed that 160 candlepower was the top limit for the carbon arc.

"I began to wonder whether there was not some other substance with a higher boiling point which could be used," Sperry remembers.

In this way the research work began. Carbides, Sperry knew, had a higher boiling point. But they were non conductors and there were, of course, a large number of different carbides. Could they be made to work at all, and which would work the best? Which would be the least costly? Sperry started to work with various methods. At last he found that in the molten state they became conductors of electricity, and in the end he found that cerium, a by-product in the manufacture of old-fashioned Welsbach mantles, suited his purpose admirably. The temperature could be forced up to 5000 instead of only 3700 degrees. This was most important as he knew that the specific emanation of light increases at no less than the fourth power of the temperature. It was necessary, however, to devise some means for using the cerium which, it must be borne in mind, works properly only after it has chemically mingled with the carbon to form a carbide. Again there came experiment after experiment. Sperry decided that the positive carbon could be made hollow and the cerium compound inserted in the hole. When the current was turned on the carbon became incandescent and the cerium united with it to form carbide in this miniature electric furnace. He designed the positive carbon in his light so that a crater would form in the end nearest to the negative electrode. And it is in this crater that extreme temperatures and enormous brilliance are achieved; as much as

1200 candlepower per square millimeter having been realized.

It was a long, tedious experiment which called for both patience and resourcefulness. Having selected the proper carbide, Sperry had to make the crater exactly the correct depth. He had to design a device for feeding in carbon at the correct speed. It was necessary to devise reflectors and lenses and mirrors, whereby such startling searchlights are now built as to develop more than 1,000,000,000 candlepower. The Sperry beacon is enabled to reach the target with no less than eight times the candlepower for the same current or watts expended, as compared with the carbon arc. This, like the rest of the inventions which bear his name, has been eminently successful. The searchlights are used in all parts of the world and by many navies. They light the paths of aviators. They are invaluable for coast defense. Not long ago after-theater crowds on Broadway were startled to see advertisements staring down at them from the clouds. A Sperry searchlight had been purchased by an advertising concern and slides fashioned, as in the old-time magic lantern. These had been thrown on low-hanging clouds and on nearby buildings.

Upon the perfection of the Sperry arc depended also the large and elaborate motion picture theaters, sometimes called "Cinema Cathedrals" by their press agents, which grace every city in the land. The old

fashioned arc did not produce enough light on the screen to make possible a very large house. And S. L. Rothafel, the famous "Roxy," dreaming of the Capitol Theater now on Broadway, went to Sperry in 1920 to see whether new illumination could not be provided for the projectors. He wanted a light so bright that the pictures on the screen would be visible to patrons seated far back or in high galleries. This was not difficult for Sperry and his associates; the new arc had merely to be mounted behind a projector. But, as in all new applications, the first trials had their uncomfortable moments since the experimental work with its occasional troubles had to be carried on with an audience of 5000 people. This was scientific work with difficulties, for the crowd in the theater was very impatient and stamped, hooted and whistled when anything went wrong. They did not realize that a new era in projection was being developed up in the projection booth. Now the Sperry arc is used in all the larger theaters. It is standard equipment, too, in motion picture studios. So great is its actinic power that "outdoor" scenes are built inside of studios and pictures are made independent of fog and rain.

Sperry is not, we have attempted to indicate, a business man. Probably the most surprising thing about his career is the fact that he has been so financially successful and this was due, perhaps, to the fact that he managed to interest business men in his

projects. Left to himself, he might wreck almost any company by drawing upon its reserves for some new experiment. This disaster is not possible at the present time, for the Sperry Development Company has been organized for his particular benefit. In this he is supreme. He receives a stated sum according to his needs. He can develop what he chooses. The rest of the business is carried on by the Sperry Gyroscope Company which he leaves in competent and trusted hands, being Chairman of the Board and supervisor of research.

Sperry is an unusually cordial person, finding great zest in life, proud of his inventions, delighted when they are praised. He recalls with particular pride that one morning, not long ago, three ruddy-faced men with a seagoing stride burst through the doors of his private office. They were, they explained, the navigator and the two assistant navigators of one of the largest liners.

"Are you," they demanded, "Sperry? Well, we wanted to shake the hand of the man who figured out 'Metal Mike.' He just brought us into port in a fog and we came right over."

"The ability to invent," Sperry has said, "is something that I feel is born in one. I am not sure that it can be acquired." But his interest in assisting young men to enter research has been shown by his willingness to add them to his staff and by his work with the National Research Council.

The Research Council, coöperating with seventy scientific and technical bodies, functions as a national clearing house of research activities. Elected in 1927 to the office of chairman of the Division of Engineering and Industrial Research, Sperry brought to the work a wealth of talent and experience in the stimulation, organization, and administration of research projects. This post offered him an opportunity to expand the scope of his activities in the interest of public service.

As chairman of this division, Sperry keeps a finger on the beating pulse of industrial development in many varied fields and is called upon to utilize the full resources of his broad experience in the conduct of its affairs.

"My work," he once said, at a dinner held in his honor, "has been a source of the keenest joy; and even though it may not have led to the goal desired, it has always led somewhere and added to both experience and caution in pushing forward. In the thrill of the work itself I have always felt that I have had my share of reward. From time to time, often after long periods of research, patient experimentation, and repeated changes, there have come great satisfactions. . . . On such occasions there comes over me a welling up from within, a sort of elation, and life takes on a new and exalted aspect. That is living! These have been my times of reward."

CHAPTER FIVE

A $30,000 CUP OF COFFEE

Samuel C. Prescott

PRACTICALLY every day for many months the "tasting squad" gathered in the women's restroom of the main building at the Massachusetts Institute of Technology. The squad consisted of about fifteen women and if any felt nervous about their duties there was no sign of it. A few were young, short-skirted stenographers. Others were dignified, more mature secretaries upon whose shoulders rested the responsibility of reminding absent-minded professors that lectures come at specified hours. There were also one or two office managers and librarians.

The lunchroom in which they gathered was one in name only, for food is not served there. The women brought their own lunches from home. But soon after they were seated each day there appeared through the doorway a man bearing a large tray with cups, cream, and sugar and two big chemical flasks filled with coffee. Often the man with the tray was Professor S. C. Prescott of the Department of Biology and Public Health at M.I.T. Sometimes his place

Samuel C. Prescott.

was taken by an assistant. Always, however, there were two varieties of coffee and the members of the "tasting squad," the name by which the women were known at the Institute, were asked to sample each kind and state which of the two they preferred.

The daily tests were part, but only part, of an elaborate series of experiments to determine how a perfect cup of coffee can be made. The cynical reader who has seen newspaper and magazine advertisements describing miraculous cigarette and cigar "tests" may hastily draw the inference that these coffee experiments were in the same category. He may wonder that an institution of the standing of the M.I.T. would countenance an investigation open to the suspicion of being, at the least, commercial. Let him be assured, however, that Professor Prescott's coffee researches were as scientific as any made in metallurgy or electrochemistry or physics. In many ways they were just as complicated and involved just as many problems. For the coffee bean as a scientist sees it is one of the most complex substances in its physical and chemical properties, ever created in nature's laboratory.

In stating that the tests were being conducted to learn the proper method of preparing the most widely used breakfast and dinner beverage on earth, one is putting the problem in popular terms. To state it more accurately, Professor Prescott was requested by the Joint Coffee Trade Publicity Com-

mittee in 1920 to plan and carry out investigations into the general chemistry of the roasted coffee bean. It might well develop, although the coffee men had so much faith in their product that they were not unduly alarmed, that the Department of Biology and Public Health at M.I.T. would find coffee a highly injurious drink, that no progress in studying the best method of preparing the beverage would be made and that the laboratory would spend thousands of dollars without visible results.

"Gentlemen," said Professor Prescott, in effect, when the committee from the coffee trade approached him on the subject, "any study made at Massachusetts Institute of Technology will be a scientific study. You cannot use the name of the Institute for advertising purposes, no matter what the outcome is. We cannot promise results. It will take us at least two years and at the end of that time we may be very little further along. Most important of all, the results of our researches will be published. For your sake, I hope they will be favorable. But they will be published, none the less."

The Joint Coffee Trade Publicity Committee drew a long breath and announced that it would take the chance. During the course of the investigation a total of over $30,000 was spent. Prescott and his associates at M.I.T. make no pretense that they have learned everything there is to know about coffee; no true research worker ever makes a statement of

that sort. They do feel, however, that definite results were achieved. They have discovered, they believe, the proper way to prepare coffee and it is significant that Prescott, when he makes the morning coffee, himself, at his home, now uses a method based on his laboratory discovery.

Countless experiments, Prescott believes, have demonstrated that coffee is not in general habit-forming. The reader has only to draw on his own experience to realize that coffee drinkers do not require additional cups, as the years go on, in order to obtain the stimulus they require for their day's work. The man who, at 21, is drinking two cups for breakfast is still drinking only two cups at 60. If anything, Prescott says, the tendency is to decrease consumption. This is true in the rare cases where caffein itself rather than caffein-containing beverages is used. Caffein does not store itself in the body and there is no evidence that it causes kidney trouble. In fact, according to the M.I.T. investigators, caffein may be less injurious to the human system than some other ingredients dissolved in the beverage when it is improperly made.

"I believe, as a result of our study and through careful examination of all the available literature on the subject," says Prescott, "that from 1 to 2 per cent of the people cannot safely drink coffee. These are extremely nervous people or have a distinct idiosyncrasy. Probably 5 per cent should be care-

ful. Our investigation indicated, if it did not fully prove, that it is not the caffein which is often harmful. The roasted coffee bean is an aggregation of carbohydrates, glucosides, proteins, fats, and waxes. Some of these, in solution, may probably be injurious. Certainly they give coffee an unpleasant taste. It is signficant to note that a friend of mine, a Philadelphia physician, has been unable for years to drink coffee. He enjoys the slight stimulation, however, and each morning he has a pinch of caffein in a glass of water. There is no harmful effect."

Prescott likes good coffee, particularly when it has been prepared in accordance with the determinations of his three years of research. He brought to the problem, then, a little of the touch of the connoisseur; he insisted that all of his "tasters" should be people with a discriminating taste, but not necessarily having similar fondness for the beverage. It is of greater importance, however, that he is a man of solid scholarship and one to whom facts are among the few vital things in life. He has been engaged in many enterprises, but he is, primarily, a teacher. His manner is that combination of diffidence and sureness so often found in the pedagogue and rarely found among the successful members of any other profession. He will interrupt any conference to talk with his students; their triumphs and difficulties are his first concern. As head of M.I.T.'s Department of Biology and Public Health he is preparing

young men to act as health officers for municipalities and bacteriologists or biochemists for industrial concerns. Many of his students enter the medical colleges, and his courses have frequently been declared the best possible training for medical work.

Prescott was born in New Hampshire in 1872, studied at Sanborn Seminary in that state, and entered M.I.T. in 1890. He was graduated four years later with the degree of S.B. from the Department of Chemistry. His most important association at the Institute was with Professor W. T. Sedgwick, then head of the Department of Biology and an international authority on public health questions. Prescott studied bacteriology under Sedgwick and for a time was an assistant bacteriologist at the Worcester, Massachusetts, Sewage Disposal plant. Teaching called him back to M.I.T. as an assistant to Sedgwick, however, and although he has interrupted this career from time to time, he has spent most of his life imparting knowledge to others. In 1922 he became head of the Department of Biology and Public Health at M.I.T. and in that capacity he has been carrying on the work begun by Sedgwick.

His work outside the Institute has the greater bearing, perhaps, on his eligibility to undertake a scientific coffee investigation. A great many of the professors at M.I.T. give part of their time to outside work, and until 1922 Prescott continued his interest in the Boston Biochemical Laboratory, which

he founded in 1905. At this laboratory many food problems were studied, products were tested and analyzed, and the laboratory early earned a reputation for telling the truth, no matter how unpleasant that truth might be. In 1912 and 1913, Prescott made several visits to the tropics and organized a research laboratory for the United Fruit Company. When the United States entered the war he became a Major in the Food Division of the Sanitary Corps and served as the officer in charge of food storage and research problems. While thus engaged he did valuable work in dehydration of foods, the object being to save space and weight in shipping by eliminating the water content and at the same time retain the food values.

Prescott would have no difficulty, then, in qualifying as an expert were he called upon to testify in court. And he was unquestionably qualified to undertake the coffee investigation in 1920. It was the coffee enthusiast in him, however, which caused him to reach his first decision; that next to healthfulness the matter of taste was of the utmost importance. He did not want to employ professional coffee tasters, experts able to tell by the flavor of a brew at what altitude the coffee used in it had been grown. He felt that they had preconceived ideas, were too rigid in their opinion. So he wandered through the corridors of the rambling central building at M.I.T. and buttonholed the secretaries of his colleagues, young

instructors, laboratory workers, stenographers, and filing clerks.

"Do you like coffee?" he would demand.

"Yes," most of them would reply.

"Will you help me in a problem I've got?" Prescott would continue.

The women known as the "tasting squad" constituted part of his force of amateur tasters. Others, men as well as women, came to his laboratory. Each day for months they were offered varieties of coffee, prepared in varying ways. Sometimes the coffee had been boiled. Sometimes it was "drip" coffee through which boiling water had been permitted to run. Often it was "drip" coffee through which water just under the boiling point had been poured. Every known kind of coffee pot—metal, earthenware, enamel, and glass—was used during the experiments. But the tasters never knew what method had been used. They were requested merely to state their preference for the one and to check the reason which turned them from another sample. The reasons given on slips which they marked and which were carefully tabulated were: "too sweet, sour, salt, bitter, queer." The members of the "tasting squad" recall that in nearly every case the samples offered in the lunchroom were more than "fairly good" and that they often had great difficulty in making their choice. They became convinced that Prescott was a coffee maker without peer.

Long before this stage was reached, however, Prescott had to organize a staff to conduct the details of the problem. As head of his department, he could give only a few hours each day to coffee. So he brought in Dr. R. L. Emerson, a physician and chemist with wide experience in foods and drugs. Dr. Emerson assumed direct charge of the laboratory work and was given three or four assistants. The first step in the study was to investigate the findings of previous workers.

"We must see the problem as a whole," Prescott told his associates. "There has been a good deal of rather half-baked theorizing about coffee. Some scientists have said that it is very injurious, but their tests were often made in hospitals among people whose health was already impaired. Others say that drinking coffee, no matter in what degree, is beneficial. They, too, rarely have authentic data."

Prescott found that a total of 671 articles and books, in many languages, had been written on the determination of caffein in coffee, the physiological effects, and the composition of the roasted bean. All of these were read and abstracts made. Only ten out of the large total had any bearing on the preparation of coffee as a beverage and none attempted to correlate the method of preparation and its effect on the health of the consumer. After the mass of published articles had been digested, this work taking all of the first summer, Prescott was convinced

that no group had gone into the matter from a sufficiently broad point of view or had, to any extent, considered the subject from the point of view of the average consumer. This he proposed to do, and with Dr. Emerson worked out his program.

"Since beverage coffee," Prescott has pointed out, "is an infusion, it follows that the composition of the liquid after infusion will depend upon the amount of the various soluble constituents which are removed by the water treatment, and is therefore directly related to the duration of the infusion, the temperature employed, the concentration or relative amounts of ground coffee and water used, the solubility of the various constituents in the ground coffee, and to the character of the materials and the apparatus used. Coffee brewing is, therefore, a complex chemical reaction rather than a mechanical process of mixing or compounding inert ingredients."

It must be obvious that Prescott faced an almost infinite variety of methods for preparing coffee. Among the factors he had to consider in preparing his brews for the "tasting squad" were (a) freshness, degree of roast, and fineness of grind of the coffee itself, (b) the character of the water, (c) the temperature of the water, (d) the character of the container used in making the coffee, (e) the time of infusion, (f) the strength of the infusion, (g) the effect of the addition of other substances.

For weeks and even months an aroma of coffee

pervaded the corridors near Prescott's laboratory. Students on their way to classes sniffed the air hungrily. Dignified professors slipped through the doors wondering whether they could wheedle a drink and always found that they could, provided they were willing to take at least two drinks and then state which they preferred. Prescott knew that he could not prepare his brews in test tubes alone, and numerous and varied coffee pots stood on the shelves. Investigation soon determined that the quality of the water used, unless it was very hard or alkaline or had been overtreated with alum or chlorine, had little effect compared with other factors. Three factors were seen to be of outstanding importance: the temperature of the water; the length of time it remained in contact with the coffee; the freshness of the roasted coffee.

The first definite conclusion reached—but only after hundreds of samples had been analyzed and tasted by Prescott's volunteer staff—was that even a brief moment of boiling resulted in strong, bitter, even astringent coffee. If the boiling continued, woody tastes were detected and the brew lost its characteristic flavor and aroma. In other words, the complex chemicals in the bean had been broken down and the beverage contained unsavory, perhaps even harmful, proteins, fats, and waxes. All of this was borne out by tabulation of the preferences of Prescott's tasters.

"A fairly large percentage," he reported, "preferred coffee which had not only *not* been brought to the boiling temperature, but which had been prepared at temperatures considerably below this point. We compared, for example, coffee made at a temperature of 85 degrees centigrade (185 degrees Fahrenheit), coffee made at a temperature of from 90 to 93 degrees centigrade, coffee made above 95 degrees but below the boiling point, and coffee which had been boiled at brief intervals such as a minute and a minute and one-half. In a large majority of cases, the preference was for coffee made at the lower temperatures, whereas coffee made at the boiling temperatures, or which had been actually boiled for some time, was looked upon with comparative disfavor."

Another subject taken up by the research workers was the effect of metals upon the taste and flavor of coffee. Coffee brewed in metal, it was soon discovered, was described as "metallic," "disagreeable," "bitter" and "puckery" when contrasted to that made in glass. Is this question of physiological importance also, Prescott asked himself? He believes that it probably is. Organic chemists have long known that organic substances and metals often combine. It has been demonstrated that caffein and mercury readily combine. Action of metals in modifying the taste of numerous food substances has been studied for years by the canning industry. And it

was, Prescott felt, obvious that many metals might yield pronounced flavors when cooked with an organic solution such as a coffee infusion. Iron has been known to do so and similar results were found with tin, aluminum, tin plate, copper, and nickel. It has been possible to detect aluminum in coffee brewed in that metal.

"We have been able to show in our laboratory," Prescott reported after the experiments had ended, "that even a short contact of a coffee infusion with metals is accompanied by the formation on the metal of a thin deposit which, we believe, is a chemical combination between some ingredients of the coffee and the metal itself. That such organo-metallic combination may yield marked flavor is well known to chemists, and this result should be of great practical interest from the standpoint of pharmacological studies."

Among the humans entitled to sympathy on this earth are those who deeply enjoy the flavor of coffee, but are unable to drink it. Their plight is the more pitiable because their number is comparatively small. They look wistfully, at breakfast and dinner, in the direction of their more fortunate fellows drinking as much coffee as they desire. Among Prescott's colleagues at M.I.T. were a number of these unfortunates and they watched, at first with skepticism and then eagerly, the researches that he was conducting. But they shrank back when, after the

work had been in progress for a year or so, Prescott invited them to sample his brews. Ah no, they said mournfully, it was quite impossible. Why, they murmured, to drink coffee would be very bad for us!

"I think not properly prepared coffee," insisted the head of the Department of Biology and Public Health. "Come on, take a chance."

Many of them did, and Prescott reports, with gratification, that without a single exception fresh coffee prepared in glass at temperatures not exceeding 95 degrees centigrade failed to injure them. To forestall accusations that their fears may have been psychological, Prescott subjected them to blindfold tests. The physiological effects were radically different when the coffee had been prepared according to the scientific method.

Three years were devoted to the work and over $30,000 was spent. But Prescott and his associates can point to definite results. Scientific research has stepped into the kitchen of the American housewife and has demonstrated that her skill, based on tradition, must give way to facts developed in the laboratory. The ideal cup of coffee, the "Prescott cup of coffee," should be made according to the following directions;

Use a glass or earthenware coffee pot. Grind the coffee fresh each time, because the bean retains the flavor best. Use any except hard and alkaline water.

Use about a tablespoonful of coffee for each cup and pour water, a few degrees under boiling, through it. Suspend the coffee in a bag or in some other way so that the grounds will not remain in contact with the water. The coffee should not be in contact with the water for more than two minutes. One minute is even better. Vary the strength by the quantity of coffee used, not by the duration of exposure to the water. Even cheaper grades of coffee will give a better flavor, if properly brewed, than expensive grades used in the old-fashioned way.

"It's easy to tell, as you come down to breakfast," Prescott says, "whether or not the cook is making coffee in the right way. If the house is filled with a delicious, appetizing odor, she's making bad coffee. Those odors should be kept in the brew, for you to enjoy when you pick up your cup."

The coffee investigation, obviously, is only a part of the work in which Prescott has been the guiding spirit. But it has brought him, perhaps, wider publicity than anything else he has done. This was impressed upon him, one afternoon, when his secretary announced that the "champion coffee drinker of America" was waiting to see him. Puzzled, Prescott found a sinewy gentleman whose profession was quantitative coffee drinking. He had been able to toss off, he assured the professor, 280 cups in 4½ hours. Prescott, vastly amused, did not forget his scientific duties.

"Do you find," he asked, "that coffee is habit-forming?"

The champion replied in the negative and added that, to him, the beverage was not harmful either. He admitted, however, that he went into careful training before engaging in a drinking contest and was always careful about his diet.

CHAPTER SIX

SOMETHING NEW

Leo H. Baekeland

TWENTY-TWO years ago the city called Yonkers, about a dozen miles up the Hudson River from New York, differed only in externals from what it is today. Its streets were illuminated by gas. Its houses, instead of being modern structures with leanings toward the Colonial or the English Cottage architectural style, boasted mansard roofs and pagoda-like turrets. There were iron dogs and deer, even an occasional ferrous bear, on the lawns. Automobiles were still regarded with suspicion and the man who invested in motor securities was regarded as a gambler soon to part with his money. The railroad at the foot of the hill had not been electrified and the Twentieth Century Limited, the wonder of the new age, roared by each afternoon leaving clouds of smoke and ashes hanging over the still river.

It was, however, a day of miracles. Hardly a week passed without a headline in the newspapers telling of some new invention. The telephone was no longer a thing of wonder. Arc lights sputtered and glared

L.H. Baekeland

in front of all the more advanced merchants' stores and Edison's new incandescent lamp had made its appearance. Men were already looking back with tolerant contempt on the century which had passed. And in one of the better residential sections of Yonkers they bowed, with respect and a little awe, when a dignified, bearded, scholarly gentleman hurried past. This was, as his neighbors knew him, "Doc" Baekeland. He was a chemist, or physicist, or something of the sort and he spent long hours in a laboratory in the rear of his home. Passing at night, the neighbors had caught frequent glimpses of the doctor, his face blue in the glow of a Bunsen burner. He seemed always to be holding test tubes in his hand. Through the window they could see, too, complicated arrangements of glass tubing and retorts.

Dr. Baekeland never had time for social evenings at whist. His only recreation was driving through the streets of Yonkers in one of the latest automobiles, a contraption he had adopted in its earliest days and whose mysteries he understood as well as did the most grimy mechanic. Asked what miracles he was attempting in his backyard laboratory, the doctor would patiently offer an explanation; but his words were largely technical and shed small light. But he was accorded courteous respect. He had already made a fortune, it was said, through the invention of Velox, a photographic paper known to

every amateur. The doctor, Yonkers conceded, was no vague theorist; by no means a crazy inventor. His laboratory even became an outlet for local pride and was often pointed out to visitors.

Being an amiable person, Dr. Baekeland may have told some of his neighbors that he was working on a problem which had interested chemists for fifty years. It is probable that he was patient when questioned regarding a somewhat depressing odor of disinfectant which hung over the lawn on humid days. What was the doctor doing, an occasional humorist asked, attempting the discovery of a new embalming fluid? To such questions, Baekeland gravely replied that he was trying to find a new varnish. He was using formaldehyde (which gave the mortuary odor) and phenol, commonly called carbolic acid. Scores of scientists had been experimenting with formaldehyde and phenol, and with substances belonging to the same chemical families. What they were seeking was a synthetic, or artificial, resin and when the reaction was conducted in a certain way they obtained something which was similar, but quite useless from a commercial point of view. Baekeland failed too, but in his failure lay achievement and wealth beyond his dreams. From his evil-smelling chemicals he produced what is known throughout the world as Bakelite.

What Baekeland had done was, in effect, to improve upon nature. He had produced an artificial

resin far superior to those yielded by pine trees. Born in a shack-like laboratory in Yonkers, Bakelite materials are to-day used in almost infinite ways. Two colorless liquids, for formaldehyde and carbolic acid are generally seen in that form, have yielded a magic substance never known before, and Baekeland has refuted the apothegm that nothing under the sun is new. In its several varieties, Bakelite resin is used for distributor heads, for self-starters, and in a dozen other places on automobiles. It furnishes highly polished tops for restaurant tables. From it are fashioned billiard balls, transparent fountain pens, gayly colored beads, armatures for dynamos and motors, dials on radio sets, insulators for high-tension electric wires, switchboards for battleships, silent gears, umbrella handles, cigar and cigarette holders. The propeller which carried the plane of Maitland and Hegenberger on their flight over the Pacific to the Hawaiian Islands was fashioned of a form of Bakelite material.

Baekeland, it might be supposed, was another of those explorers whose wandering and aimless steps brought them around a bend and into the sight of valleys that gleamed with treasure. But this is not —romantic as such explorers may be—in accord with the facts. How did you happen, his friends have inquired, to stumble upon so interesting and profitable a research problem as that of the synthetic resins?

"I did not strike it haphazardly," Dr. Baekeland has answered. "I had looked for just such a subject for a number of years until I found it among the many lines of research which I undertook in my laboratory. More than once it has happened that at the end of years of work on some particular subject or another I had come to the conclusion that what I had found was not worth while following up further, although it might appear attractive enough to other chemists. So, more than once, I abruptly closed my work on some subject and took up another line, until finally I succeeded in finding a subject on which I could feel thoroughly enthusiastic."

The Baekeland of 1906, less than fifty years old, was internationally known and moderately wealthy. He had developed Velox paper and had sold his rights to the Eastman Kodak Company. He had worked with Elon H. Hooker of Niagara Falls on the industrial development of the electrolytic cell invented by Clinton P. Townsend, and a huge plant had been built to produce electrolytic soda. He might have retired to a life of ease and have indulged his pleasure in boating and motoring. Instead, he turned to the problem of synthetic resins, a subject in which he "could feel thoroughly enthusiastic." He was at no time embarrassed for funds, being well able to engage assistants and purchase materials. All that he needed was infinite patience to wrestle with "the innate cussedness of inanimate things."

He had to venture part way down one path only to find that it led nowhere, to retrace his steps, to break down barriers, to find paths through uncharted labyrinths. But Baekeland had plenty of patience and in the end he won. To-day, the Bakelite Corporation operates a number of manufacturing plants where the raw materials are made. For the production of finished articles there are numerous factories in the United States working under license.

Baekeland had done what many another chemist had been seeking, during all of his spare time, to accomplish. It was not long before another fortune was potentially his. So complex was the chemical nature of his discovery, however, that scientists continued to work on synthetic resins. It seemed wholly logical to suppose that some other path to the same destination might be discovered and a similar product, which did not infringe the patents held by Baekeland, created. But the pioneer was good-naturedly tolerant of their efforts. An executive of a large industrial concern came to him one day, obviously distressed.

"Doctor," he said, "you know, of course, that every organic and most inorganic chemists in the country have been trying to work out a new synthetic resin. One of our young men says he's got it and has asked our permission to file patents. Now you've been square with us and we use large quantities of your Bakelite materials. It didn't seem quite

fair, to me, to go ahead with this. I said I'd put it up to you."

"Have him try it out," said Baekeland. "If I can beat you in a patent fight I'm that much stronger. If I can't, you and your men are entitled to what you have found."

There is something akin to the persistance of the Old World craftsman in the patience with which Baekeland solves his chemical problems. He might be a silversmith of the days when men worked with their hands and gloried in their skill, or a glassblower from Venice. He differs, of course, in that he works with his mind. But the attitude is the same, and this may be because he was born and educated abroad. Leo Hendrik Baekeland was born in the ancient Flemish city of Ghent on November 14, 1863, of parents who realized the value of education and who arranged that he was to go through the university. He attended the elementary schools, a government high school and entered, at the age of seventeen, the University of Ghent. He seems to have won academic distinction without difficulty and graduated four years later with the degree of Doctor of Science and at the head of his class. Subsequently he became professor of physics and chemistry at a government normal school and, in 1889, an associate professor at his Alma Mater. There was not, it appears, much surplus money in the Baekeland family because the son found it necessary to support himself while

in college. This he did without difficulty, by tutoring his less brilliant fellow students.

Like most of his fellow explorers in the realm of science, Baekeland was an excellent scholar and a few years later he won a competition held among the alumni of the four most important Belgian universities. A committee of senior professors of chemistry of the four institutions acted as the jury, and Baekeland received a gold medal and, far more important, a traveling scholarship which enabled him to study at universities in England, Germany, and Scotland. He was also able to visit his future home, the United States, in 1889. This visit was the turning point of his life. Except for it he would, in all probability, still be a university professor.

Baekeland is not inclined to discuss, except with a few of his closest intimates, those days of hard work and intellectual growth. And yet it would appear obvious that he was a young man with a swift, inquiring mind, a somewhat serious young man who saw the objective of life rather clearly and who resented intrusions, whatever their nature, upon his attention. One is inclined to believe that he tramped over the low, flat Flemish landscape without noticing the mists that swirled in from the sea or the green trees of spring or the white buds against the hillsides. Instead, while walking each day from the campus of Ghent to his home nearby he was occupied in translating an American novel from English into

German. Years later, wealthy and a leader in the field of industrial science, a friend called at his office to ask for a subscription to a carillon for the restored tower of the Louvain Library. The committee in charge, said the friend, expected a donation from Baekeland, for had not the doctor, himself, grown up in a country where the sound of bells rolled across the fields at sunset?

"Too many bells!" said the doctor with pleasant gruffness as he reached for his checkbook. "They were going all the time, whenever I tried to work."

"My real intense education," Baekeland once remarked, "began only after I had left the university, as soon as I became confronted with the big problems and responsibilities of practical life; this education I received mainly in the United States, where for twenty-seven years I was thrown into contact with so many varied subjects. I hope to remain, until I die, a postgraduate student in that greater school of practical life, which has no fixed curriculum and where no academic degrees are conferred, but where wrong petty theories are best cured by hard knocks."

Such is the philosophy, expressed in one way or another, of all these men who have achieved distinction in the field of research. It was General John J. Carty, of the American Telephone and Telegraph Company, who said, for instance, that the engineer is an "advocate for truth" whose "works must be

tried in the inexorable court of nature, where no errors are committed and no exceptions granted."

The youthful Baekeland who was teaching in Belgium had not yet learned these axioms, however. He says, himself, that he was "just as cock-sure as some of my older colleagues that everything was as simple as it appears in some of the textbooks." It was an interest in amateur photography which undermined his scientific smugness, for here he found that science did not understand what happened to silver bromide (used in the emulsion of plates and printing papers) when exposed to the light. Something happened, obviously, for silver bromide after exposure, gave up metallic silver upon treatment with a developer. It still looked just the same. Chemically, it seemed identical. Under the microscope no change was apparent. Before leaving Ghent for the United States in 1889, Baekeland had gone fairly deeply into this and other mysteries of photography and he learned, thereby, the first rule in the code of the research worker:—things are usually different from what they seem.

Since this chapter concerns, chiefly, Baekeland's work with synthetic resins, his experiments with photographic papers can be touched on only in passing. What he did, striped of technicalities, was to invent a silver solution relatively insensitive to the yellow rays of the spectrum. This meant that it could be developed a short distance from artificial light after

being exposed a few inches away [illegible] He had arrived in New York from Ghent and had made the acquaintance of Richard A. Anthony, of E. and H. T. Anthony and Company, which later became affiliated in the Ansco Company for the manufacture of photographic supplies. Anthony offered Baekeland a position as chemist and he accepted this, informing the Minister of Education in Belgium that he would not return to his post at Ghent. After two years he left the Anthony firm and became a consulting research chemist.

It was in 1893 that Baekeland went into partnership with Leonard Jacobi, of Yonkers, and founded the Nepera Chemical Company for the manufacture of his new paper, which he had christened "Velox." He had been working on this for at least ten years, even during his student days at Ghent. But it was while attempting to make a living as a consulting chemist that he learned another truth in the code of the industrial scientist: it does not pay to attempt too many things at one time.

"I tried to work out," he has explained, "several half-baked inventions, the development of each of which would have required a small fortune. Fortunately for me, I was taken out of this muddle and shaken to my senses by a very severe illness which nailed me to my bed for several months. While I was hovering 'twixt life and death, with all my cash gone, and the uncomfortable sentiment of rap-

idly increasing debts, I had abundant time for sober reflection. It then dawned upon me that instead of keeping too many irons in the fire, I should concentrate my attention upon one single thing which would give me the best chance for the quickest possible results."

Baekeland is an intensely practical scientist. He would, one gathers, be rather slightly unhappy among those of his fellows who labor in the realms of pure science. He wants to be certain that results are ahead. In the vapors that arise from his test tubes he sees the tall chimneys of great industrial plants, and in the bubbling of a retort he visions boilers that will drive his machinery. And yet, paradoxical as this might seem, his methods are those of the theoretical research worker. In him, too, there is a streak of the philosophical. His reasoning processes, one gathers, are those of a Socratic dialogue. Is this path I am following likely to lead somewhere? Am I attempting too many things? Is there any use for such new creations as I may find? His tendency toward pondering these matters crops out in many of the public addresses that he makes. He is, on these occasions, likely to warn his associates against building "castles in the air on processes which are not beyond their laboratory stage."

He made such a speech in February of 1916 on the occasion when he was presented with the Perkin Medal, annually awarded by a joint committee of

American chemical societies for the most distinguished service in industrial chemistry. He told of the difficulties experienced in persuading photographers to use Velox, his new paper, of how amateurs and professionals had been accustomed to using the sun instead of artificial light and were reluctant to attempt experiments. He had never been, he said, "more impressed with the fact of how routine holds sway over this world" and added that he was "rather stubborn" in his own point of view and persevered. He then warned his auditors that any process developed was of slight value until it had become a "commercial paying enterprise." He had sold Velox to the Eastman Kodak Company because he had built its manufacture into a prosperous business. Otherwise he would have received comparatively little. He pointed to the necessity of selecting the proper associates for new chemical enterprises. Many scientists, he said, "make reckless connections with almost anybody who can furnish them the first money, regardless of whether they are proper persons to help or advise or inspire them in their work." It was during this address that Dr. Baekeland told of turning to the subject of synthetic resins only after giving deep thought to its possibilities. He also remarked that insufficient capital had brought ruin to many a chemical manufacturing business and expressed regret that banks and other financial institutions did not more often have on their staffs

scientific men, competent to pass on technical problems. This had long been the custom in large German banks and it was one of the reasons for that nation's industrial preparedness.

There can be little doubt, then, regarding the state of mind of Baekeland when, in 1906, he began his work on synthetic resins; he intended to find something of definite value. His first step, of course, was to learn everything which had been done in this field. As far back as 1871 the German scientist, Adolph Bayer, had observed and written about the behavior of phenols and aldehydes (the various members of the carbolic and formaldehyde families) when heated together. With some of his associates, Dr. Bayer had found the subject fascinating—but even more annoying. Queer results were constantly obtained. Instead of crystals capable of purification, a shapeless inert mass came from the reaction. During the thirty years that followed, numerous other scientists studied this problem, but they achieved slight success. Some obtained resins of a sort, it is true. But they were very much like ordinary resins and when heated they formed shapeless, porous, hard, infusible, insoluble masses which seemed to have no value since they could not be made into usable forms.

Baekeland did more than read about the failures of the men who had gone before him. He repeated their experiments, step by step. In 1891 Kleeberg was able to use formaldehyde itself, since it had come

on the market as a commercial product, and it was he who had produced the infusible, insoluble mass. Baekeland, still seeking a new shellac, decided to work on this. Night after night he would retire to his bed, weary and discouraged. His chief assistant, Nathaniel Thurlow, told him that it was useless. The stuff would *not* dissolve, it would *not* melt, it was perverse, nasty, unreasonable. Then came the Great Idea. If this stuff, Baekeland must have reasoned, is so stubborn, cannot that apparent vice be made its virtue? If he could control this refractory substance, would he not have a new and superior product, unlike anything seen on the earth before? Would it not be possible to control the reaction so that the result in its final form would be hard, strong, and unchangeable?

He redoubled his labors and the lights in the laboratory behind his Yonkers home burned even longer into the night. He used every solvent he could obtain, impregnated woods with the stuff, attempted to carry on the reaction within the fibers themselves. He met with failure after failure; to his chagrin, the reaction defied his skill. But apparently it never occurred to him to abandon the work. He learned, at least, that only under certain conditions would the reaction of formaldehyde and carbolic acid produce anything of value. He wanted workable firmness and, at the same time, the valuable properties of the fluid state. What he needed was control, command

of the process, the ability to predict what would happen at every stage of the reaction.

Baekeland studied, it must be remembered, the failures that had gone before and he took advantage of these to the fullest extent. Acids, used in the reaction, produced solubility and fusibility. But bases guaranteed final hardening and reduced the gaseous activity. Now he was getting somewhere. He tried adding minute quantities of ammonia, tried caustic soda which reduced the tendency to foam up under heat. He tried every base he could think of. For weeks, now, he went to bed jubilant instead of weary and discouraged. Each morning might bring the final victory!

He turned once again, to the notes left by his distinguished predecessors. Heat, they had written, was the creative agent. But if too much heat was used the activity was increased and the chance of control lessened. Use only 50 to 75 degrees centigrade, they warned. But Baekeland knew that there was another answer. The tendency to foam, to make trouble, had been decreased by the use of ammonia. Would not pressure complete the cure? If a thing foamed, the thing to do was to place a counterpressure on it. So Baekeland used compressed air in his oven, and increased the heat to 150 to 200 degrees centigrade. He had won! The liquid had changed to a transparent solid like amber, clear and beautiful. It formed perfectly in its mold. But it

was much stronger than amber. No matter how much it was heated, after it had become hard the first time, it would not melt again. It "froze," so to speak. It would dissolve not at all. It was a poor conductor of heat and a worse one of electricity. Here was a material for which the electrical world, seeking a new nonconductor, had been waiting. But it had many other possible uses, and Baekeland, essentially conservative, thought that forty industries might be his customers. To-day hardly an important plant in the world fails to use some form of Bakelite material.

It was on the night of February 6, 1909, at the Chemists' Club in New York, that Dr. Baekeland announced his discovery. Reporters were present, but they naturally did not grasp the significance of the event. The next day, in the New York *Sun*, a headline stated that Baekeland "Claims Much for Invention." The *Herald* explained that the new material was a synthesis of coal-tar products and staggered its readers by announcing that the official name was "oxybenzyl-methylenglycolanhydride." Patents had already been obtained, of course. The first resinoid put on the market was the original pure one made into pipe bits, beads, and bottles; for holding hydrfluoric acid which, as every high school chemistry student knows, eats into glass containers. Bakelite is transparent and can be colored in a variety of ways. The liquid resinoid products which

soon appeared could be used as a varnish or a lacquer. It kept metals from corroding.

As a molding material the resinoid could be "mixed" with wood fibers, placed in molds and heated. The molded object takes the exact shape of the mold and has a beautifully polished surface. Metal inserts may be imbedded during the molding operation and hand-labor costs reduced. Paper and cloth can be impregnated with the liquid resinoid, and the result is both strong and tough. From this product are fashioned gears for use in machinery where silence is an essential. Airplane propellers, so hard that they are not shattered by bullets, are made from layers of canvas treated with Bakelite and then subjected to heat and enormous pressure. Its resistance to electricity makes Bakelite an excellent material for switch-boards. Bakelite played a vital part in the World War and was used on all Liberty motors, the radio, machine guns, airplane propellers, depth bombs, and as a varnish for shells.

The research which Baekeland started in the complex field of synthetic resinoids has not ended; at the laboratories of the Bakelite Corporation at Bloomfield, N. J., it is still going on under the direction of Dr. L. V. Redman, whose knowledge of the subject is at once ramified and authoritative. New ways of using the astonishing material are being worked out, improved methods of production. Young men from the technical universities are taken into

the laboratory from time to time, and the work goes on. As president of the Bakelite Corporation Baekeland continues to play an active part. The years stretch behind him as milestones of success—marked by achievement based on a willingness to work, a persistence that rarely faltered, a belief that practicality was as necessary as pure scientific research. Baekeland is sixty-five and now he is willing to be less directly occupied in the laboratory until some new problem arises to stimulate his imagination. When not at his New York office he divides his time between his home in Yonkers, and Miami, Fla., where he lives, strangely enough, in the house once owned by William Jennings Bryan, challenger rather than champion of science. Yachting is one of his chief diversions. He will set sail into the face of a storm in a small boat, aware that cooking will be virtually impossible and eating difficult. A guest taken on one of these trips went into the galley during a blow and found Baekeland bending over the stove attempting to fry some eggs. Just then a large wave struck the boat and he was pitched into a corner, the eggs in his lap. He calmly picked them up and ate them with relish.

Baekeland does not look his age, partly because he has shaved off his beard and now wears only a small mustache. In appearance, he still looks not unlike a college professor. He is dignified, well-read, courtly, and has an excellent command of Eng-

lish although he speaks with a slight accent. The honors always accorded to leaders in scientific life have been his, of course. He has been president of the Chemists' Club; vice president of the Society of Chemical Industry; chairman, New York Section, American Chemical Society; president, American Electrochemical Society; president, American Institute of Chemical Engineers. He is Honorary Professor of Chemical Engineering at Columbia University, a member of the Naval Consulting Board, of the Advisory Committee on Chemistry of the Department of Commerce, and member of the National Research Council. He has published many papers on scientific subjects and has been awarded a half dozen medals for his discoveries.

And yet, although he is successful and wealthy, Baekeland does not survey the world with complaisant satisfaction. The philosophical note grows pronounced and often pessimistic with the years. He seems to wonder, not infrequently, what mankind will do with the tools placed in its hands by science and he has given warning that the next war will be horrible beyond belief. There will be, he says, no noncombatants. Gases and bombs will wipe out whole cities. But science is not to blame for this. On the contrary, "what progress the world has made . . . it owes directly or indirectly to exact science." There is, in his mind, no warfare between science and theology. What the world needs, he said at another

time, "is more of a plain, generous week-day religion of deeds rather than a Sunday school religion of words."

"It needs less hyprocrisy, haughtiness, lying and suspicion, and more decency and good will among people."

W D Bigelow

CHAPTER SEVEN

A LABORATORY ON WHEELS

Willard D. Bigelow

ONE morning a year or so ago, the owner of a large cannery in one of the Middle Western states stood in his warehouse and gazed with satisfaction on rows upon rows of neatly stacked wooden cases. In each were a dozen or more cans of corn, and the canner, reflecting that the quality of his pack had been excellent and its sale assured at good prices, was anticipating the profit he would make. He placed his thumbs in the armpits of his vest and rocked back and forth from his heels to his toes. It would bring, he reflected, a very neat profit.

His pleasant thoughts were rudely interrupted, however, by the breathless arrival of his foreman from another warehouse.

"Your pack's swelling!" the man said.

"Swelling? Impossible!" the owner retorted. But he hurried to the warehouse and found that the foreman was right. From the cases stacked in rows upon each other with one side open, he took several cans, and found that the ends were bulging. Immediate-

ly he wondered whether the sterilizing process had been continued long enough. Could it have been that the process temperature was not high enough? No, he felt certain, there had not been any slip-up in either of these things.

Whatever the cause, something had brought hazard to his entire pack. How far it had gone he did not know, but as he continued to dig in toward the center of the mass of cases, piled ten feet high on the warehouse floor, he found more and more bulged cans. The profits of which he had been dreaming a moment before seemed likely to vanish in thin air.

In the midst of his alarm he happened to recall that the research laboratory of the National Canners' Association had some men on a field trip not far from his plant. Perhaps they could help. Anyway it was worth trying. Within a short time he had them located, and was assured by Willard D. Bigelow, director of the research, who was then with the party, that a quick trip would be made to his cannery.

When the research director arrived he opened a number of cans and examined the corn. Then he started to ask questions regarding the canning plant's methods. Nothing seemed amiss, except that it developed that the cans had been placed in the cases without being cooled sufficiently, and the cases were stacked together in such a way that the cans, except those on the outside of the stack, would be many hours in cooling.

Under these conditions, Bigelow reasoned, spoilage bacteria known as thermophiles, which are unable to grow at a temperature below 100 degrees Fahrenheit, would have opportunity to develop and cause the spoilage evidenced by the bulging cans. He suggested that the mass of cases in the warehouse be rearranged in tiers with air spaces between to permit heat to escape rapidly. His diagnosis was correct. The bulging of cans stopped. The bulk of the pack was saved from spoilage. Since that time, this canner has always stacked his cases this way and he has had no trouble.

The ordinary consulting bacteriologist or chemist, no matter how skilled he may be, rarely has enough practical knowledge of the canning industry to be of much help in an emergency of this sort. He does not know what questions to ask the canner when something has gone wrong. But the field men of the National Canners' research laboratory bring to these problems all the knowledge gained by the laboratory's fundamental work on the principles and methods of processing. Under Bigelow's direction these studies were developed until, as a final step, it was determined to find the source of spoilage bacteria and to study conditions in canning plants that led to their growth and multiplication. So it was that members of the staff were sent into the field, first of all to plants that had already experienced spoilage. Laboratory facilities were necessary in this

work, and portable field laboratories were taken to a number of canning plants. But these were not very mobile; besides, transferring them from place to place was slow and expensive. Finally, in the summer of 1927, the laboratory men went out in automobiles from one of the portable laboratories and thus were able to use its facilities in studying neighboring plants.

A member of the staff then conceived the idea of building a laboratory on wheels, a well-equipped research laboratory to be mounted on an automobile chassis. Here was a new idea; or rather, the old idea of Mohammed and the mountain applied to the practical use of research.

The canning industry is composed of relatively small units, and the modern tendency is to locate canning plants in communities where the raw products can be grown most advantageously. This has resulted in a wide distribution of the industry. Some of the problems of these canning plants can be studied satisfactorily by a distant laboratory; others can be studied only by a laboratory on the spot. Wide and constant touch with canning-plant problems, it had been found, not only aids the laboratory in the direction of its fundamental research work, but makes it easier to provide individual service to canners.

With association members located in the remote canning districts and scattered over half the growing

area of the United States, and the canning season moving from place to place with the rotation of the crops and complicated by the vagaries of the weather —to meet all these conditions was a large order! How to move the laboratory and staff to the scene of operation and keep it moving was the question that was put up to Bigelow and his staff. A laboratory on wheels was their answer. Putting the idea into practical form, however, required some extended planning and study.

Investigation disclosed that a passenger bus admirably adapted for carrying the field laboratory equipment was for sale at a reasonable price; it was purchased and fitted up. The mobile laboratory started on its journey in the summer of 1928. On board it were two bacteriologists, an engineer familiar with canning machinery, and a technician.

The laboratory first rolled into Wisconsin when pea canning started, and carried on its work not only for the plant at which it was parked, but also for a number of other plants that could be reached by automobile. Operations finished at one point, they moved to another and in the course of the season a number of widely separated states were covered. How much this service meant financially through the correction of faulty conditions and the prevention of spoilage it is impossible to guess, much less to estimate accurately. It is certain, however, that the moving laboratory is one of the most economic ways

by which industrial research has aided the industry, and has resulted in large economics.

Bigelow, now an authority on the refinements of scientific canning, had intended to be a geologist. But during his senior year at Amherst College, in 1889, he was on a field trip with some fellow students and discovered that some of them were able to distinguish rocks, geological formations, and other objects at much greater distances than himself. Believing that this would place him under too great a handicap, he decided to take up other work. He had been majoring in chemistry, in addition to geology, so he chose this as his vocation. At first he planned to teach and accepted a post as assistant professor of chemistry at Oregon State College. Then he went to Washington, D. C., to teach in the high schools. He came into contact, while at the Capital, with chemists in the government service and joined the Department of Agriculture in 1892. By 1901 he was chief of the division of foods in the Bureau of Chemistry and was associated with Dr. Harvey W. Wiley, the pure food pioneer. This rugged and militant advocate of pure food left an enduring memorial in the inspiration implanted in the breasts of young hopefuls of those early days. The Pure Food and Drug Act was passed in 1907 and part of Bigelow's duty was connected with the enforcement of its provisions.

"I soon reached the conclusion," he remembers,

"that it was not enough merely to prosecute violators. I felt that we should do something which would really assist the food men to improve their products and thereby make it possible for them to observe all of the provisions of the new law. The Bureau of Chemistry was authorized to engage in research, and I decided to investigate the methods of the canning industry, largely because it was the largest compact food industry. I inspected a great many of the plants and studied their processes, and in 1912 some of the canners asked me whether a research laboratory would not be a valuable thing. I told them, of course, that it could be enormously valuable and that careful study might improve their product by making it more uniform and lessening spoilage. A year later they asked me to resign from the government service, and the research laboratory of the National Canners' Association was established."

In 1913, Bigelow recalls, the canning industry had already passed from its secrecy stage. However, men now living can remember when little was known about the underlying physical and chemical facts; when processes were still controlled by the "process man," so called because he took general charge, and when there was much mystery about it, encouraged by the process man because it increased his value and his pay. But those days were over by 1913, and the canners had already banded themselves for common interest and mutual advantage in the

National Canners' Association. It was realized that the problem of one was the problem of all, and that the canners' future depended on their ability to turn out a wholesome product of unvarying quality.

Processes used in the industry at that time were the result of experience—what the canners termed "cut and try" methods—and on the whole they were surprisingly good. There were deficiencies, however, which in some cases led to spoilage and in others resulted in a product inferior in quality to what can now be obtained. These deficiencies could be cured only by a scientific study of the technological processes, and it is this that the research laboratory has supplied. From the very first the laboratory has had the coöperation of the canning industry as a whole; without it, success could not have been attained.

Canning, you may be aware, is not one of the oldest of the industrial arts, like tanning or textile weaving. But it has a fairly extended history. Late in the eighteenth century, with Napoleon's star fast rising, the French government offered an award of 12,000 francs for the best method of preserving foods. Napoleon well knew the value of foods not subject to wastage or spoilage, and in the campaigns that were to come he wanted to prevent losses of this kind. François Appert, a French confectioner and brewer, was attracted by the bounty and began work. It was 1809 before he had evolved an acceptable

method. In that year he was given the prize. Appert used wide-mouthed bottles sealed with corks. The food introduced into these bottles, covered with water and then heated in boiling water. Appert believed, this being before Pasteur's discoveries, that the success of this method was due to the elimination of air. His method achieved a certain commercial value, and in 1819 Ezra Daggett and Thomas Kensett, of New York City, were packing oysters, lobsters, and salmon. In 1820 William Underwood was canning fruits and sending them from Boston to South America. These men were supposed to have studied Appert's method in Europe. Glass was used almost exclusively in the early days of the industry, but in 1810 patents were granted in England for making tin cans. In 1839 they were being used by Underwood in America. By 1860 the industry was growing rapidly, and a certain amount of canned food was used in the Civil War.

Meanwhile Pasteur had proved that the spoilage of food was due to minute forms of plant life, particularly bacteria, yeasts, and molds. These are always present in foods, multiply, and cause changes known as spoilage. The food must, then, be cleansed of these organisms, which is done with heat, and they must be protected from further contamination. In canning, the order is reversed. The food is first sealed in the can to prevent further contamination, and then the cans and contents are heated to a tem-

perature which will kill the existing organisms. Yeasts and molds are easily destroyed by comparatively low temperatures. Bacteria are, however, more difficult to handle and their destruction is the chief objective of canning and preserving.

"Bacteria," Bigelow explains, "in spore form, corresponding in many ways to the seeds of large and complex plants, present numerous complications to the canner. Although some are destroyed by a brief exposure to boiling water, others must be boiled for thirty or forty hours. Increasing the temperature beyond that of boiling water decreases the time period. In order to achieve temperatures well above 212 degrees Fahrenheit, pressure kettles, tightly sealed receptacles, were designed. In these, canned foods are heated by steam under pressure. Canners call the sterilizing operation 'processing.'"

Until research was begun under Bigelow's direction, the processes used were largely arbitrary. If the canner found that he was having more spoilage than usual, he increased either the temperature or the duration of the treatment. If, for a considerable period, he was experiencing little spoilage, he was likely to shorten the process so that the appearance of the food would be improved and so that he would reduce costs by saving fuel. Sometimes, quite unexpectedly, the canner would have trouble although he had been using an identical process for

many years. Then he was likely to blame it on the tin plate he was using.

At the main laboratories at Washington, D. C., elaborate tests were begun in an effort to remove processing from the realm of chance. It was felt that if exact information regarding what went on could be obtained, the haphazard craft could be given a scientific standing. With his assistants, Bigelow began to study the heat penetration of canned foods, the heat resistance of spoilage bacteria, the influence of acidity on processing, and the source of spoilage bacteria. On the first of these problems, heat penetration, some work had been done prior to 1918, but all of it had been on a laboratory scale and the results were not applicable to canning conditions. In 1918 an apparatus was developed which made possible the determination of heat penetration in cans located in any part of a pressure kettle during the commercial canning of foods. In this way the distribution of heat in the kettle could be studied and comparative tests made.

"Research methods, apparatus, and basic principles successfully applied in one industry, may be utilized in an entirely different application for the purposes of another industry," said Bigelow, in a reminiscent mood. "Do you know how I got the original idea for the apparatus which now measures the heat penetration in the processing of canned foods? Well, it's interesting, particularly now as

I look back on it. Simplicity itself. Our laboratory at the time was coöperating with the American Can Company and the American Sheet and Tin Plate Company in a study of tin plate, and with members of the staff I was at the mill where the plate we were studying was being manufactured. We were making full observation of the manufacturing process under varying conditions, including the temperatures at various parts of the oven in which the steel sheets were being annealed. Throughout the night it was necessary to make observations at intervals of, first, half an hour, and then an hour. The temperatures, which varied from 1200 to 1500 degrees, were taken by means of a unique arrangement of thermocouples, and between readings I had a lot of time to think. It was then that the idea came to me of applying the basic principles of this apparatus to the measurement of heat penetration in canned foods."

Heat, one is told in elementary physics, circulates by convection and conduction. Heat is transferred by convection many times as quickly as by conduction. In a can of water, for instance, the portions nearest the wall of the container rise within the can and mix the contents; thus heat is carried to all parts by convection. But when the contents of the can are only semifluid or substantially solid, convection currents are impeded.

Peas, for example, are canned with a dilute solution of sugar and salt, and convection currents

move almost as readily as in water. The peas only slightly impede the currents. With beets, convection currents also move readily, but because of their size the beets exert a greater cooling action on the liquor surrounding them so that the heat reaches the center of the can more slowly. Even in a can of beets, however, heat quickly reaches the center. But when products like spinach are closely packed in a can, convection currents are almost entirely cut off and heat penetration proceeds to a greater extent by conduction. The more closely the spinach is packed in the can, the slower the heat penetrates. With products consisting entirely of solid material, like sweet potatoes, and with products containing a considerable amount of starch, like cream style corn, convection of currents are practically eliminated, and heat penetration is by conduction.

All this, obviously, is valuable to the practical canner, because it is vital for him to get sufficient heat to the center of the can to destroy spoilage bacteria without heating the outer portion of the contents to such an extent as to impair its quality or appearance. From exhaustive studies of heat penetration for different products, the laboratory has worked out processes for various foods, and these have been made available to the entire industry.

Heat penetration has, of course, one chief purpose—the destruction of bacteria. So the laboratory began to study the heat resistance of the

organisms responsible for food spoilage. Bacterial spores, particularly, had never been studied with this objective in mind. A large number of samples of spoiled canned foods were analyzed in the laboratory and the bacteria responsible for the spoilage were isolated. The cultures obtained afforded an excellent collection of spores which were resistant to heat. They had survived the ordinary processes of the canning industry. They were the type with which canners had to contend and which had been responsible for large losses.

Before the laboratory began its work on heat resistance of spoilage bacteria, it was well known in the industry, through experience, that acid products, such as fruits, tomatoes, and kraut, do not require so high a pressure as nonacid vegetables, fruits, and meat. Years ago Pasteur had recognized the fact that the amount of acid present influences the amount of heat necessary to destroy bacteria. Canners, however, did not know that the acid played a major rôle, but they did know in a general way what processes were necessary for each product. The laboratory developed a great deal of information about the influence of acidity, and it has shown that spores will not germinate, nor will spore-forming bacteria grow, in products having the acidity of certain fruits, tomatoes, and kraut.

From the mass of data obtained, Bigelow and his assistants attempted to determine, for the use of the

canning industry, the time and temperature needed for processing any particular canned food. But in some canning plants spoilage bacteria seemed to multiply more rapidly than in others and for this reason it was important to study their source. A systematic bacteriological survey of the canning industry was undertaken. It had long been assumed, for instance, that certain heat-resistant bacteria with which pea and corn canners had to contend, came from the soil. It was believed that they were brought into the cannery from the farms on the raw product. Investigation disclosed, on the contrary, that they were present in small numbers in sugar added to the food in canning, and conditions were found in some canning plants which led to their multiplication in abnormal numbers.

It was discovered, for instance, that wooden tanks used for preparing and heating the syrup sometimes became "infected." The spores entered the pores of the wood, where they continued to grow. It was also learned that pockets or traps in pipes and machines hold portions of the food long enough to permit the spores to develop.

Now, when trouble develops, samples of the canned product are sent to the laboratory for examination so the cause of spoilage may be determined. If this examination indicates it is due to some condition in the plant, a member of the laboratory staff makes a visit to the plant and, if needed, the traveling laboratory is called into service.

All the data obtained as a result of research since the laboratory was first established are at the finger tips of the men traveling with the automobile laboratory each summer. Meanwhile, the main laboratory continues its chief work, which is to study fundamentally the causes of spoilage and obtain information which will enable canners to avoid it.

"A reputable lawyer," says Bigelow, "tries to keep his clients out of trouble so he will not have to defend them when trouble arises. A reputable physician gives his first attention to keeping his patients in good health rather than to curing them after they succumb to illness. The laboratory of the canning industry is actuated by the same principles. This attitude is understood and cordially appreciated by the canners of the country, who have given the research work their utmost coöperation."

A branch laboratory in San Francisco studies the problems of canners in the Pacific Coast region. Another laboratory at Seattle gives its chief attention to problems of the salmon canners. These far-flung outposts of the main laboratory, while not mobile, are constantly reaching out for new worlds to conquer in the realms of research.

The entire staff of all these laboratories, restless with the spirit of research, is already charting, under Bigelow's direction, new areas of the unknown and making plans for the next expedition as explorers in the field of canning science.

John A Mathews

CHAPTER EIGHT

INOCULATING IRON

John A. Mathews

JOHN ALEXANDER MATHEWS, a vice president and director of research of the Crucible Steel Company of America, is one of the most methodical men alive. Portly, mild mannered, low-voiced, totally lacking in outward distinction, he follows a rigid system in the routine of his daily life. He commutes every day from his home at Scarsdale in Westchester county to his office in New York City. He takes the same train each morning, usually sits in the same seat in the smoker, lights a cigar and unfolds his New York *Herald-Tribune*. He reads the paper systematically; from the first page through the news columns, the editorials, the sporting page and the financial news. By the time the train is approaching the Grand Central Terminal he has finished the paper.

It is interesting to note, then, that Mathews' position in the steel industry is due, at least in part, to sheer chance. He took a scientific course at college because he did not wish to become a minister,

lawyer, or doctor, a grave peril to those studying academic subjects nearly forty years ago at his Alma Mater. He did advanced work in chemistry only after he had been unable to obtain a job as reporter on a Pittsburgh newspaper. He avoided metallurgy in the nineties because he believed that it did not rest upon a scientific, but only on a descriptive basis. Eventually he found himself, without quite realizing it, working under scholars in this country and England who had accomplished miraculous things with steel and iron alloys. He had decided to adopt teaching as his life work and when he went into industry to gain practical knowledge he found himself in the employ of a steel company. He has never gone back to teaching but has always considered himself an educator. Other men renowned in the world of science have known from their early years that chemistry or physics or engineering was to be their calling and have set up laboratories in the cellars or kitchens of their boyhood homes. But Mathews, apparently, had barely glanced at a test tube until he reached college.

He passed his early boyhood on a farm in Wisconsin and observed in the laboratory of nature the ways of the birds and the bees; he learned about the planting and cultivation of crops and helped make the cider, soap, and sorghum, and learned the chemistry of these processes fifteen years later under Professor Charles F. Chandler. He acquired habits

of industry and thrift and regrets that his children have been deprived of the same opportunities.

To-day Mathews is internationally known for his developments of high-speed and noncorrosive steels. He has been able, quite literally, to "inoculate iron" against acids, alkalis, salts, and against fatigue by perfecting alloys in which comparatively small quantities of chromium, molybdenum, nickel, silicon, manganese, and other materials, in varying combinations, are used. Every housewife is familiar with the "stainless steel" knives now available for cutting grapefruit, tomatoes, and oranges, which do not become black or otherwise discolored after being used once or twice. Obviously, however, but a small quantity of the modern steel is used for knives. The bulk of the production is being used in industry. Every automobile would cost at least five times what it does at present were it not for the hard high-speed steels which make possible stamping, cutting, and other steps in large-scale production. Steels which do not corrode are used for tanks and mixing machines in chemical plants, for pumps in mines where water reeks with sulphur, for blades in steam turbines where oxidation is always a factor to be fought, for periscope tubes and many submarine parts, even for dental plates and bridgework. In automotive and oil-engine valves the new steels withstand high temperatures, as well as oxidation and scaling.

"But there is no universal alloy good under any

and all attacks," Mathews pointed out not long ago. "In fact, misapplications may sometimes have a humorous aspect. For example, recently a sample of noncorrosive steel, which was known to resist strong acetic, phosphoric, sulfric, and nitric acids was submitted for some application in a distillery across the border. It was a failure, and the reader can form his own conclusion as to the possible effects upon the human metabolism of bootleg liquor!"

So well is Mathews known at the present time for his work with noncorrosive steels that his part in the development of vanadium alloys has been forgotten, even by some of his fellow metallurgists. Vanadium, a silver-white metal which seems to have derived its name from Vanadis, the surname of Freya, the Scandinavian goddess, was for almost a century after its discovery exceedingly rare. It was kept in the cabinets of museums, among other unusual metals, and did not have any particular use. It had been discovered in Mexico in 1801.

Many early research workers experimented with vanadium to determine whether it had properties which might be valuable. But they accomplished little. In 1900, however, Professor Arnold of the University of Sheffield, in the cutlery center of England, discovered that a steel containing a small quantity of the mysterious metal could be shaped into tools with marvelously keen cutting edges. Here was a real use for vanadium, and men scoured the

earth to find ores containing it. In 1900 its cost was greater than that of gold. Two brothers, J. J. and J. M. Flannery, of Pittsburgh, had heard of the new and potentially valuable metal and they started, like prospectors for gold, to see whether they could find some. Their task was infinitely difficult for they had to search over virtually the entire known world to find deposits. In the end they discovered a huge store, in a form known as vanadium sulphide, but it was located far up in the Peruvian Andes, at an altitude of over 16,000 feet. The brothers brought their ore down the side of the mountains on the backs of llamas and later by means of small railways. They had to convince the steel industry that their metal was of value and this, too, proved enormously difficult. Most of the American steel manufacturers had not even heard of vanadium. They did not propose to waste money in experiments, and for a time the Flannerys even had difficulty in reducing their ore to basic metal. In the end, however, they were able to perfect a process for this.

Meanwhile Mathews, teaching chemistry at Columbia University, had decided that his value as an instructor would be increased by the acquisition of practical knowledge and in 1902 he accepted a post as metallurgist for the Sanderson Brothers Steel Company at Syracuse. He had heard of Arnold's experiments and determined to see whether vanadium could be used in the newly discovered high-speed

steels. Making inquiry in 1902, Mathews found that vanadium was being sold for $75 a pound. But the next year the price had dropped to $40, a few months later to $12.50. At this price Mathews purchased about half of the country's available supply, thirty pounds, and paved the way for the successful operations of the Flannery brothers and the development of the Vanadium Corporation of America. Mathews was the first to introduce the use of vanadium in high-speed steels. This was in 1903. The resulting alloys doubled or tripled the efficiency of high-speed steels of the day, and soon metals could be cut at a speed so great that the tool glowed with a red heat yet did not loose its cutting edge.

It was several years prior to the formation of the Vanadium Corporation of America that Mathews directed the attention of Henry Souther, of the Automobile Licensed Manufacturers' Association to the possibilities inherent in chrome-vanadium, nickel-vanadium, and nickel-chrome-vanadium steels and supplied the first samples the motor industry ever saw. At about this time, 1906, the price had dropped to $5 a pound. To-day vanadium is an essential element of nearly all high-speed steel tools fashioned, from it are built tractors for the farm, automobiles for city streets, and guns for the dark days of war. Its resilient and long-wearing nature makes it invaluable, also, for the machines themselves, machines that must run at high speed and absorb enormous

shocks. Except for vanadium steel Lindbergh could hardly have thrilled the world by winging his way across the sea to Paris.

"The flight of Lindbergh," Mathews has said, in pointing to the relation between steel research and developments in machine design, "was a great exhibition of personal stamina, courage, and confidence made possible by generations of science, years of skillful engineering, and the conscientious manufacture and intelligent selection and use of materials for which we chemists may modestly take some credit and press on to greater things in days to come."

Mathews was responsible, too, for other innovations in the early days of his work with steel. The Halcomb Steel Company, also located at Syracuse, N. Y., had in 1906 placed in use the first electric furnace in this country. Several types had been invented in France, Sweden, and Italy and had been experimented with for a number of years. The electric furnace proved of great importance, due to the growing automotive industry. For the first time it was necessary to produce high-quality steels in tonnage lots, and the old crucible steel-melting process was hardly suitable for quantity work. In a few plants good alloy steels had been obtained through open-hearth operations, but the number of plants doing this work was limited and the open-hearth product, in general, was not good enough. The electric furnace could best be developed on the

basis of years of experience in crucible processes. Mathews, coming to the Halcomb Steel Company as superintendent in 1908, found that trouble was being experienced with the electric furnaces. Patiently and with persistence, drawing upon his knowledge of crucible operations, Mathews and his associates made the furnaces operate according to schedule. With constantly increasing demands from the automobile manufacturers, clean steels—free from sulphur and other impurities—were produced. It required, according to Mathews, "patience, skill, experimentation, and study." At one time, he admits, they were almost ready to give up the process.

It was, I have said, largely chance which brought John Mathews to the field of chemistry and later to the alloy steel industry. Perhaps it would have been more accurate to say that heredity was responsible, since three generations of his ancestors were gold and silversmiths. It is this, probably, which causes him to be fascinated by the historical aspects of metallurgy. Collecting old books on ancient methods for extracting precious metals from their ores is a hobby and he has one of the few original editions of *De Re Metallica* written by Georgius Agricola in 1546. This Latin work was first translated into English by Mr. and Mrs. Herbert Hoover some years ago. Mathews owns many other valuable books on metallurgy and magnetism. He believes, for the man over forty, fads are not foolish, hob-

bies not harmful, and collecting no crime, but he prefers old books to old bottles, and early American silver to postage stamps.

The Mathews family lived for several generations in southwestern Pennsylvania and were of the sturdy pioneer Scotch-Irish stock that contributed so much to the religious, educational, and industrial development of that region. His father spent his whole business life in Washington, Pennsylvania, and here the future research director of the Crucible Steel Company of America was born, May 20, 1872. His father died before he was three years old. His mother, Frances Sage Pelletreau, of early Colonial Huguenot ancestry, was a native of Southampton, Long Island. In 1843 she migrated from Massachusetts by stage coach to Washington, was graduated from the famous old seminary at that place, and taught French and music in Ohio and Kentucky for several years before her marriage. When left a widow with four young children she removed to a farm in Wisconsin and remained there until her youngest son was ready for preparatory school. Mathews attended Washington and Jefferson College, founded by pioneer missionaries in 1787 for the purpose of providing a Christian education for young men and to prepare them for the ministry. Any one who did not choose to study Greek was looked upon as only partly educated. Mathews chose a scientific course but without any very great urge

to become a scientist. Like many other youths, trained as he was in his boyhood, Mathews discovered when he began studying science that there were discrepancies between the traditional teaching of religion and the new science. He delved into the matter and after burrowing beneath the husks of dogmatic theology found the simple truths of the Great Teacher revealed in unsuspected beauty, and thereafter for him science and religion were friends and not foes. While at Washington and Jefferson he supported himself in part by selling newspapers and then by writing for them. Gaining facility in this, he covered sporting events at the college for Pittsburgh papers and evolved his own system of shorthand scoring for baseball games. It was probably through his work as a reporter that Mathews gained a skill with the pen unusual among men of science. He is almost alone in his ability to state a technical problem in clear, understandable language well within the comprehension of the layman. This is noticeable in all of the many papers he has written and is apparent in the addresses that he makes. He cannot help being aware that his brother scientists get snarled in a mass of technicalities when they attempt to explain their work and he feels that this is a real handicap to the advancement of research.

"The American public," he said at the 1926 dinner of the Engineering Foundation, in New York, "seems to accept the results of scientific achieve-

ments in material things, but pays scant attention to science itself. The late Professor Robert S. Woodward, returning from an academic function at Cambridge University, told me he had observed that 'in England science is respected, but in America it is merely respectable.' In advertising language, we have not 'sold' science to the public. Either our great men are unable for the most part to state scientific truths in readily understandable language, or else they feel it beneath their dignity to do so. In England, the popular scientific lecture is seen at its best, and no man is so great that his dignity suffers by lecturing at the Royal Institution or the Society of Arts, or even by giving lectures to children during the Christmas holidays. Can you imagine our children thus spending their holidays?"

This gift possessed by Mathews, a talent for expression through the written word, strikes those who do not know him well as all the more surprising because, in conversation, he is not particularly impressive. He talks very little. But with his pen he can turn a neat phrase, and in making a plea his arguments are most effective. He objects, for example, to the passion for standards and specifications which grips many manufacturers. These, he believes, are often meaningless, arbitrary, ridiculous. One customer of the Crucible Steel Company has written on the bottom of every order "This must be perfect steel." And no steel mill, Mathews said, "is likely

to make steel that cannot be rejected (particularly on a falling market or long inventory)." Good sense, he insists, is desirable in writing standards and specifications because, once written, "they acquire a sort of sanctity, like the Ten Commandments or the Constitution before the adoption of the Volstead Act."

Mathews might have succeeded, it will be seen, as a writer of editorial paragraphs, even as a column conductor, on a newspaper. But, to return to his history, he was unable to obtain even a reporter's job on a Pittsburgh newspaper. He decided, consequently, to become a teacher and went to Columbia University in 1893; he was later an assistant in assaying, a tutor in chemistry, and a candidate for a Ph.D. He received his degree in 1898, having proved himself so able that he was awarded a fellowship for his last year. Then he was given the Barnard Fellowship for the Encouragement of Scientific Research. He went from Columbia to the Royal School of Mines at London. There he studied with Sir William Roberts-Austen, and in 1901 returned to Columbia to work with Henry Marion Howe.

Both Howe and Roberts-Austen were interested in alloy steels and did lasting work in adding to the fundamental science concerning them. Howe was a pioneer in the development of manganese steel, and Roberts-Austen, knowing that Mathews had worked under the Columbia professor, took unusual interest

in the young American. From his association with these two men grew Mathews' interest in alloy steels. He is to-day one of the greatest authorities on so-called austenitic steels, named for Sir William Roberts-Austen. These are alloy steels noted for their resistance to corrosive attack, coupled with good qualities under fatigue and tension tests and excellent resistance to oxidation and scaling at extreme temperatures.

Mathews, while abroad, was awarded the Andrew Carnegie Scholarship by the Iron and Steel Institute of Great Britain. Returning to America, he spent a year with Howe and in 1902 he became metallurgist and later manager of Sanderson Brothers, of Syracause, a subsidiary of the Crucible Steel Company. He was soon made superintendent of the melting department and in that capacity encouraged the development of vanadium alloys. In 1908 he transferred to the Halcomb Steel Company, became its president and general manager seven years later, and in 1920 returned to the Crucible Steel Company. The burdens of management pressed down on him. Primarily a scientist, even to-day a man who sometimes regrets that he ever abandoned teaching, Mathews was made president of the company in 1921. He accepted the responsibilities until 1924, constantly improving the quality and variety of the steel turned out. In that year, however, he resigned to permit himself to give more time to

the work he really enjoyed, scientific research. He resigned from the presidency, consented to play a lesser part in the management in the rôle of vice president, and resumed direction of the laboratories. During the war he had served on several technical committees, particularly for aircraft production, and had been chairman of the Aircraft Engine Forgings Committee. To this work he gave his detailed knowledge of steel and its characteristics.

The march of science is no less swift in some of the branches of metallurgy than in such fields as radio and telephony. But the obstacles in the path of progress were probably greater than in industries not handicapped by tradition. The steel industry had long been in the hands of craftsmen who worked with sure, certain touch, who produced excellent metals without having the remotest idea of the underlying scientific facts. Twenty-five years ago the industry had just developed the high-speed steels without which the wheels of modern manufacturing would all but cease to turn. As recently as 1912 noncorrosive steels were virtually unknown except in the laboratory. The owners of large chemical plants would not consider using them, so great was the initial cost. For years they were deaf to arguments that steels which would resist acids, oxidation, salts, and enormous temperatures would, in the long run, save far more, in lowered upkeep and depreciation, than their cost.

"Conservation as regards corrosion is receiving belated attention," Mathews has declared. "The total loss due to corrosion is an almost inconceivably large item. Figures as high as from $2,500,000,000 to $3,000,000,000 per annum have been suggested as covering the stupendous loss, including not merely the corroded metal, but expenditures for protective coatings, replacements, shutdowns, and labor. The public is not yet alive to the possibilities of conservation through the use of noncorrosive ferrous alloys. The researches of the past decade have yielded the necessary products, but their adoption has been very slow. They are relatively expensive, but some day the public will realize that first cost is unimportant if only parts which do not rust or wear can be had."

To illustrate the economies of the new steel, Mathews tells of a coal mine in the Middle West where the management had grown weary of replacing pump shafts which lasted from a few days to a few weeks. Finally shafts made of nickel-chromium-silicon noncorrosive alloy were installed, a steel known as Rezistal developed by Charles M. Johnson. After constant use for three and a half years, during which few renewals of packings were required, the mine was flooded for six months. When the water had been pumped out it was found that the rails, spikes, trolley hangers, air tanks, and many parts of the pump itself had disintegrated but that the shaft was bright,

clean and unstained. It is still in operation in another pump.

"To replace in the public mind the basic assumption that 'steel rusts' with another formula, 'some steel does not rust,'" says Mathews, rather plaintively, "is no small undertaking. The steps of evolution are sometimes slow and painful."

If I give the impression that Mathews poses as the originator of alloy steels, I am doing him a grave injustice. In nearly every address that he makes, in every paper he writes, credit is given to the pioneers who have gone before; to such men as Brearley in England, B. Strauss in Germany, Elwood Haynes in the United States. A little later came Maurer, Monypenny, C. M. Johnson, Becket, and Armstrong. Mathews, being an intensely practical person, indulges in no high-flown oratory concerning the objectives of research. In the steel industry, "as in all other lines of manufacturing," its principal purpose is "the increasing of profits." New and more wonderful steels will be developed by men looking into microscopes, bending over electric furnaces, braving the burning heat of the open hearth. These will find their purchasers and the company supporting the laboratory can show, in the long run, greater dividend returns.

The research department of a steel company may function in a variety of ways, Mathews points out. It may attempt to improve the product, thereby

leading to greater demand, larger output, lowered costs, and greater ability to meet competition. It may work for lower costs by finding cheaper raw materials, by improving old processes, by reducing labor costs, by developing new products and uses for by-products. Research in the steel mill may improve operating conditions and effect savings in power and in fuel.

"All of these," Mathews says, "result in public good and bring their reward because they lead to better goods, cheaper goods, or the better supplying of the public need. If it is commendable to make two blades of grass grow where one grew before, how much more worth while is it to make one pound of steel go where two went before, by improvement in quality or treatment or the adaptation of a steel particularly suited to the requirement.

"Not much exception would be taken to this program for a research department by any board of directors or executives, but there would probably be a difference of opinion as to how far research should be extended into the field of so-called pure science in a money-making institution. In my judgment and experience, the pure science of to-day is the applied science of to-morrow. I have been impressed many times with the fact that discoveries of supposedly only theoretical value soon come to be applied to the practical affairs of manufacture. When I first started in the steel business I made a

practice of reading every word of Sauveur's *Metallographist* and of the Journal of Iron and Steel Institute of Great Britain. I was working in a strictly crucible tool-steel plant, and at that time there was practically no literature bearing upon crucible or tool steel. I was amazed how often the reading of articles even on nonferrous metallurgy gave me ideas applicable to my work.

"It is not my judgment that the pure science of the steel industry should be left to the college laboratory and the Bureau of Standards, nor do I believe the practical problems of the industry should be turned over to these institutions. The industry should assume all of its practical problems and most of its theoretical ones, leaving the impracticable for thesis subjects, and then the graduate will not have so much to forget when he enters a plant. Most of my university work was in the line of organic chemistry and I do not regret one minute of it. My efforts to visualize molecular changes in carbon compounds have led me to visualize the idiosyncrasies of the carbon conditions in steel, and when I see steel in the course of manufacture or heat treatment I have a mental iron-carbon diagram pasted right on the piece like a tool-steel label."

One is inclined to wonder, in the light of all this, whether Mathews is quite as practical as he believes himself and whether his interest in science, itself, is not greater than his interest in the steel industry.

This interpretation might be placed, at least, on his disinclination to continue as president of the Crucible Steel Company. The men who know Mathews will insist, in fact, that he is by no means the somewhat obvious person that he appears to those who meet him at luncheon or dinner or at some scientific meeting. There are, these men say, contradictions which stand in the way of fully understanding him. His pioneer heritage, for instance, causes him to be outwardly disdainful of the medals and other distinctions which come his way. He has the reticence of men who wrenched a living from the rocky soil of a Vermont farm. Secretly, though, he is said to be greatly pleased that his fellow scientists take occasion to do him honor.

He enjoys his home at Scarsdale and finds satisfaction in the work of the Presbyterian church of which he is a trustee. He is devoted to tennis and plays an excellent game, despite the fact that he is a heavy man and rather older than most of those who take such violent exercise. With his family, garden and books, he lives a quiet, placid life—a trifle set in his ways, perhaps, but finding it very pleasant. If he is assailed with doubts at any time, his friends say, it is regarding his abandonment of teaching as a profession. Sometimes he grows weary of cost sheets, tonnage records, and the other details of industrial management which still occasionally confront him, although as vice president he has far

more time for science. On these occasions he wonders whether, at some day in the future, he cannot retire and establish his own laboratory where new wonders may be uncovered.

But all directors of industrial research do that, from time to time.

A D Little

CHAPTER NINE

THE VOICE OF RESEARCH

Arthur D. Little

If Arthur D. Little did nothing except talk, or write an occasional paper, he would still be contributing his share to the cause of scientific research. He is the despair and the envy of his associates for he possesses a gift desired by all of them, although many deny it. This is the ability to write clearly, smoothly, even with literary distinction. While other scientists are helpless unless they quote mathematical formulæ, Little charms his readers by quoting the classics. He can explain how paper is made from wood pulp and do it so cleverly that the owner of a chain of newspapers, who uses tons of paper but knows nothing of its manufacture, can understand him. He can convince a group of bankers that an industry which encourages research is far more safe, financially, than one that does not and can do it without once mentioning a chemical or physical process. Little can, and does, tease funds from multi-millionaires for scientific schools and colleges by persuading them that the future of mankind rests upon research or the study of technical subjects.

I do not mean that all the other leaders in the field of research are totally helpless with the pen or on the platform. We have seen that John A. Mathews of the Crucible Steel Company is more than moderately skilled and there are others who can write and speak so that their readers and hearers have an even chance of knowing what they are talking about. But Little, the founder of Arthur D. Little, Incorporated, chemists, engineers, and managers, of Cambridge, Mass., is the acknowledged master. In view of his attainments in the field of science, one pays him a dubious compliment, perhaps, to say that he is best known for his writings. But this, I think, is true. He is almost certainly the only important person engaged in industrial research who is frequently asked to contribute articles and book reviews to that most erudite of all popular magazines, *The Atlantic Monthly*.

To forestall any hastily formed conclusion that Little is merely a dilettante writer or speaker, it is well to point out that he is a chemical engineer, attended the Massachusetts Institute of Technology, and is a holder of the honorary degree of Doctor of Chemistry from the University of Pittsburgh. He is the inventor of processes for the manufacture of chrome-tanned leather, artificial silk, electrolytic manufacture of chlorates, and, with his associates, of methods for the production of a long line of alcohols and special products from petroleum. He is

a specialist of international reputation in the chemistry of cellulose, paper making, processes of fiber treatment, and in gas and petroleum. His studies of industrial wastes have saved untold sums and have kept many a corporation from the ignominy of bankruptcy.

The esteem in which Little is held by the members of his own profession in this country is evidenced by the fact that he has served both as president of the American Chemical Society and as president of the American Institute of Chemical Engineers. That this esteem is international has recently been conspicuously demonstrated by his selection as president of the Society of Chemical Industry of Great Britain.

Among other scientific societies of which he is a member are the Chemical Society (London), La Société de Chimie Industrielle (New York Section), American Academy of Arts and Sciences, American Institute of Mining and Metallurgical Engineers, and American Society of Mechanical Engineers. He is vice chairman of Engineering Foundation, the research agency of the four great national engineering societies.

He is a life member of the corporation of the Massachusetts Institute of Technology and a member of its visiting committee for the Departments of Chemistry and Chemical Engineering. His interest in the Institute, as well as in chemical education, was demonstrated some years ago when he submitted to

its Corporation a plan for a coöperative course in which M.I.T. would unite with forward-looking manufacturing plants in the training of chemical engineers.

The essential novelty of the plan lay in its recognition of the fact that any process of industrial chemistry is merely a coördinated series of what Little termed "unit operations," which, when once mastered by the student, in all their variations, could be arranged in proper sequence to meet the requirements of any process. The proposed course was, therefore, one for the intensive study of these unit operations as conducted in the plants themselves. It was approved by the Corporation, but no funds for its establishment were available. Little thereupon went to Rochester and secured an interview with Mr. George Eastman, of Eastman Kodak Company, who quickly recognized the possibilities of the plan and provided the endowment of $300,000 with which the school was started. It is now known as the School of Chemical Engineering Practice, and Little shares with Mr. Eastman the honor of its foundation. He is, however, insistent that the credit for bringing the school into effective, organized operation belongs to his friend and former partner, Dr. William H. Walker, then Professor of Chemical Engineering at M.I.T. It should be emphasized that the system differs in important respects from that found in many engineering colleges where the

men merely obtain practical experience by working in industrial plants during vacations or for certain hours each day. Three field stations, where access to several plants is available, have been established. The men in the school devote all of their time, even when in the plants, to educational work and therefore receive no pay from the companies which are coöperating. Most of the men are either graduates of M.I.T. or of other engineering colleges. Only ten or twelve students are in a group and an assistant professor is in charge of each.

So much for the professional standing of A. D. Little. His success as a consulting research chemist is due, primarily, to his technical ability. And the final factor is Little's personal presence. We have taken occasion to point out errors in the popular conception of industrial explorers, that they are *not* uniformly shabby, unkempt, vague gentleman who stumble from their laboratories to find themselves incongruous figures in the sunlight—as conspicuous, for their lack of tailoring, as a farmer in town to sell his oats. And yet it cannot be denied that many of them are known as gentlemen despite their apparel. But Little, like L. H. Baekeland of Bakelite fame, is the motion picture director's idea of a prominent banker, suave, well-dressed, poised, at ease when addressing a dozen capitalists or five hundred members of a scientific society. No odors from his laboratories cling to his clothes. Whether he really

does so or not, he seems to be the type who would always wear a dinner jacket after 7 o'clock in the evening and who, like the traditional Englishman, would carry a bathtub in the heart of Africa. Although Little is sixty-four years old, no heaviness of middle age has caused his figure to sag. Of medium height, he holds himself erect. He smokes expensive cigarettes. His voice is low, well-bred, and his choice of words is good. But although he has lived in Boston most of his life, and on his father's side comes of an old New England family, he has not a trace of the ultra-refined, and so frequently synthetic, Boston accent. He is, I should say, rather better-looking than most men of his age. White hair, almost beetling white eyebrows, a good mouth, further give the impression that he is a sophisticated clubman.

Little's interest in chemistry began more than fifty years ago and was the result of a rash investment on his part. He was born in Boston and spent the first year or two of his life within the walls of Fort Independence in that city. His father had been an artillery captain in the Civil War and had been given an easy assignment at the fort to recover from wounds he had received. After a few years the family moved to Portland, Me., and Little first went to school in that city. One day, when he was about twelve years old, a boy seated behind him nudged him.

"Have you a dime?" the boy whispered.

Little fished through his pockets and discovered that he had just that and nothing more.

"Give it to me," the boy behind him went on, "and I'll show you some chemical experiments."

Little consented, and passed over his entire wealth. That afternoon the two boys stopped at a drug store and with the dime purchased a piece of glass tubing and five cents' worth of sulphuric acid. Reaching his classmate's home Little saw, for the first time, the miracle of sulphuric acid reacting upon zinc and producing hydrogen—only in this case the generator had been improperly set up and exploded, quite harmlessly, after a moment or two. He also witnessed a piece of marble hissing under the action of the acid, and found that blowing through lime water created a white deposit. That night, tremendously excited, he hurriedly sought his father.

"I'm going to be a chemist," he announced.

From that day the boy was definitely certain of his life calling. Everything that came into his hands was a subject for experimentation. He studied the stars with a concave mirror and one night announced, to the astonishment of a neighbor who knew something about astronomy, that he had "discovered the moons of Mars." As this was a year before Hall, the neighbor was skeptical and it later developed that the tiny stars which Little had seen in his miror, one on each side of the planet, were due to multiple re-

flection. It is easy to imagine that Little became a good deal of a nuisance as his interest in chemistry grew. Like every other boy consumed by his passion, he mixed together every chemical on which he could lay his hands and the results were weird, odorous, and explosive. It is probable that the same gods which watch over drunkards, guard youthful chemists. At all events Little was never hurt and this despite the manufacture of hand grenades out of beer bottles. He filled the bottles with an explosive mixture and then tossed them over a fence, listening with anticipation for the crash of the explosion.

His family gladly coöperated in his desire to become a chemist but felt that he ought not neglect all other learning in preparation for science. So he began taking an academic course at the Portland high school. This did not hold his interest and he was only a mediocre scholar. Later, however, he came in contact with an excellent and discerning science teacher who gave him a great deal of individual attention. Little became a prize student and so proficient in chemistry that he became the proud assistant in charge of the apparatus for this teacher at public lectures. He went to the Berkeley School, in New York, to complete his preparatory education and made ready to enter M.I.T., the school from which seven out of ten research directors appear to have been graduated.

At Tech, Little gave most of his time to his studies although he participated in many student activities and was freshman editor and later editor-in-chief of the college magazine. Years afterward he founded the *Technology Review*, now one of the best magazines of its kind. Financial pressure caused him to leave college after three years and he did not graduate with the class of 1885, as he had hoped to do. Instead, he took a summer course in paper manufacturing at Amherst and looked around for a job. His first position, which he has described as "part chemist and part clerk," was at a pulp and paper mill at Rumford, near Providence, R. I. Six weeks later he was appointed superintendent of the pulp mill, the first in the country making sulphite wood pulp. During the winter of 1885 he was sent to Newbern, N. C., to start another sulphite mill and his place at the Rumford plant was taken by Roger B. Griffin who the next year became a partner in Griffin and Little, of Boston, consulting chemists.

When the laboratory of Griffin and Little was opened in 1886 it was located in an ancient, dingy building on Milk Street. The landlord, Little recalls, attempted to insert a clause in their lease providing that all clients must walk down from the laboratory on the sixth floor. And although the landlord was persuaded out of this, his elevator service was erratic and unreliable and many of the first customers of the new firm found that they had to

climb the entire six flights. Little is still wondering how much business he lost when the elevator was out of order. The entire capital possessed by the two young chemists was $2500 and with this they bought what equipment they could afford. Certainly their outlook for success was gloomy enough. There were six men in the city of Boston who were trying to make a living by the commercial practice of chemistry and each was pessimistic regarding its possibilities. One of them, a graduate of Harvard and of a German University, volunteered that he had not made $700 during any one of the eleven years that he had been practicing. Not only in Boston, but generally in the United States, chemists were viewed with distrust. It was widely believed that their reports were framed to suit their clients. It was charged that they took commissions for recommending products, processes, and equipment. So underpaid were they that unless they enjoyed private incomes they could not usually maintain social contacts with important and wealthy members of the community, men who might be in a position to provide work.

In 1886, $5 was the prevailing fee for a sanitary analysis of water, and with this analysis the chemist was expected to give an hour or so of advice on anything that happened to interest his client. Even experts, men who had been engaged in the practice of chemistry for years, were greeted with derision if

they asked fees of more than $25 a day for their services. The top price for analyzing a sample of sugar was 75 cents and Griffin and Little abandoned sugar tests when, one morning, a composite sample representing 6000 tons was brought to them to be examined at this rate. Competition among the chemists in Boston was keen and their woes were increased by frequent price cutting.

Griffin and Little seem to have been insane optimists. As they opened their small laboratory with an office eight by twelve feet in size, they told each other that at least $14,000 would be taken in during the first year. But at the end of the twelve months they discovered that their income exceeded their expenses by only $600. Somewhat sadly, the two young men took the $600 and divided it. Their confidence was unshaken, however, and Little, who even then was a master salesman of research, hurried out to get additional clients.

"When I opened a laboratory in Boston," Little has recalled, "the street cars were drawn by horses, and I remember the clang of the first electric cars on Boylston Street and the consternation they caused among their equine competitors. From my window on Beacon Street 2000 bicycles an hour could be counted, where now more automobiles pass. I have seen the fishtail burner supplanted by the Welsbach mantle and the incandescent electric lamp develop

from carbon to tungsten filaments through to the white light of argon-filled bulbs.

"When I first began the study of chemistry we were taught that there were certain permanent gases. They were called permanent because they could not be liquefied, but, almost before I had learned the lesson, Pictet and Cailletet had liquefied oxygen. There are now no permanent gases, and liquid air has become a commonplace of the laboratory and the raw material for great industries."

Little was reminiscing in this vein in an address a few years ago before the Division of Engineering and Industrial Research of the National Research Council and much of what he said on that occasion demonstrates how very recent are most of the important developments in science. The American Chemical Society, for instance, had only 300 members against 15,000 in 1926. Students were taught that the atmosphere contained only oxygen, nitrogen, aqueous vapor, and a little carbon dioxide. But within a comparatively few years patient research in a laboratory had uncovered five previously unknown gases: argon, helium, neon, krypton, and xenon.

"Even their names," the erudite Little explained, "carry interest and suggestions: argon, the lazy one, because it forms no compounds; helium, because the spectroscope had revealed its existence in the sun before its discovery on earth; neon, the new one; krypton, the hidden one; and xenon, the stranger.

But already the lazy one has been put to work in incandescent lamp bulbs; helium, with nearly the lifting power of hydrogen and noninflammable, has become the key to the safer navigation of the air by dirigibles; while neon tubes flash advertisements in shop windows and assist chauffeurs to locate engine trouble."

We seem, however, to have abandoned Little, the rising young chemist. The first few years were ones of bitter struggle during which his creditors could probably have plunged him into bankruptcy had they all asked for payment at one time. But the partners managed to scrape together enough capital to buy out one or two other consulting chemists and for a time, in addition to a miscellaneous practice, did the major portion of the sugar testing in Boston. This was arduous work and paid very badly. It was the custom to bring in samples at 6 o'clock in the evening and demand a report by 9:30 o'clock in the morning. Thus the work had to be done at night.

By 1893 the young firm seemed to be making progress, with $375 in business being recorded each month. But that year brought a grievous loss when Griffin, the senior partner, was killed by an explosion in the laboratory. Terribly shocked, and feeling that his personal loss was far greater than his professional one, serious as that was, Little determined to carry on alone. He did this from 1893 until

1900, and each year his income increased a little and his reputation a great deal. He had made such progress by 1900 that Dr. William H. Walker of M.I.T. consented to the formation of a partnership, and for the next five years the firm was known as Little and Walker, Consulting Chemists and Engineers. In 1909 Dr. Walker withdrew, and the company known as Arthur D. Little, Inc., Chemists, Engineers, and Managers was organized. Within two years the business volume was eight times what it had been in 1904. Meanwhile the laboratories and offices had been moved several times, always with additional space and finally with several floors. In 1917 the business was institutional in its character. Little had clients in all parts of the world and had piled up a moderate fortune. In that year he began work on the palatial building he now occupies. This is a large structure with three floors and a basement. It looks over the Charles River basin and adjoins the grounds of M.I.T.

Institutions such as Arthur D. Little, Inc., occupy a unique position in the field of research. Many of their customers are hard-headed, so-called practical, business men. Suspicious of research, inclined to be contemptuous of men who work with microscopes and test tubes, these men not infrequently discover that they are falling behind in the competitive race, that their ledgers are showing an unaccountable number of red ink entries. Having attempted to

spur sales by advertising, having discharged a few plant managers and hired new ones, having engaged efficiency experts, they are likely to turn, in the end, to men of Little's type. Obviously, he must be able to explain his methods in language that is nontechnical, clear, and convincing. And he requires that his associates be able to do the same. Little is still the master salesman and he still finds that his most difficult task is to find among his scientist-subordinates men who can also tell the story of research. The laboratory on the banks of the Charles is notable for its nontechnical atmosphere in the midst of the most technical activities. The layman visiting the Little laboratories feels less inferior, less out of place, than in probably any other laboratory in the country.

It is a rash visitor to Arthur D. Little, Inc., who insists that something cannot be done. It is the objective of Little and his associates to accomplish the impossible. Several years ago a client was holding forth on the uselessness of a certain raw material and remarked, without great originality, "You cannot make a silk purse out of a sow's ear." "Oh, can't I?" Little appears to have answered. And to prove that ancient sayings, apparent impossibilities, and tradition will not stand in the path of industrial research he proceeded to do just that, to make a purse out of a sow's ear. From the gelatin and tissues contained in a lady pig's ear his associates pro-

duced a red and blue tasseled purse, and to-day a prized exhibit in Little's museum is a board on which is mounted a sow's head. Beneath it are jars containing the materials taken from the ear, in the various stages of artificial silk manufacture. In a glass case at the bottom of the board is the purse and on each side of the exhibit are affidavits from packers attesting to the truth of it all. Needless to say, the layout has a powerfully chastening effect on skeptical industrialists inclined to insist that research cannot aid their failing concerns.

There are, of course, many and varied laboratories in the Little plant and in addition to a large general scientific library, another devoted to petroleum, gas, and coal. Probably the unique feature is the fact that much of the equipment is semicommercial in size. It includes a complete pulp and paper mill and for special studies in petroleum there was built in the yard an oil refinery of semiworks size. The customers of Arthur D. Little, Inc., desire to see processes translated into industrial terms and to know whether laboratory methods can be adapted to plant operation. Little can demonstrate, before the eyes of his clients, that the process which works in the laboratory will, if he says so, be just as successful in the factory. An engineering department of the company makes available mechanical and civil engineers who will assist in the designing of plants and factories and assume full responsibility for them.

Accountants are ready to make financial audits. They can do even more than this—make "chemical audits." These determine the efficiency of manufacturing processes, the quality of supplies and raw materials, the wastes and their prevention, the status of equipment, even the future of the business itself.

That Little has been able, to a degree probably unequaled by any other chemist, to translate the theoretical into the commercially practical is indicated by a brief summary of more recent developments. His laboratory has discovered processes for the manufacture of vegetable glue from starch, the recovery of turpentine and resin from yellow pine stumps, the extraction of zinc from complex ores, the separation of potash from complex saline deposits, the concentration of phosphate rocks. His laboratory staff developed a process for manufacturing a special mulching paper, used in Hawaiian sugar cultivation, from the waste products of sugar mills and built a mill at Olaa, H. I. Elaborate studies have been made in fuel processing and conservation and both the Boston Elevated Railway Company and the Edison Electric Illuminating Company have been clients. The Little organization developed the dry mats—necessary when German imports ceased during the war—used in newspaper stereotyping and saved large sums for newspaper publishers. They have worked out many processes of paper manufacture and, most recently, a practical method for

making newsprint paper from Southern woods which, when operated on a large scale, promises to effect an important reduction in the cost of newsprint.

It must be clear that Little has been singularly successful in the profession which he selected, as a boy of twelve, back at school at Portland, Me. The significance of Little, it seems to me, lies in his unusual talents as a salesman of the idea that prosperity must depend, in the long run, on research. Few could criticize Little if now, past sixty, he devoted all of his time to the business of which he is the head. Its responsibilities, with a payroll of more than $1000 a day, are heavy. He has demonstrated on many occasions, however, that he is willing to preach the gospel of research, to give time to expounding its virtues and the necessities for it. His talents for doing so have earned him a wide audience.

Among all the addresses he has made there are two which are quoted from time to time, whenever research workers gathers. One of these, "The Handwriting on the Wall," was made before the American Institute of Chemical Engineers three years ago. He told the story of Belshazzar's feast and the writing in which Daniel read the ruler's doom.

"I am willing," he said, "to interpret the handwriting which confronts our industry. It reads: *The price of progress is research, which alone assures the security of dividends.* . . . The future of the shoe industry in New England has long been a matter of

local concern, but it would be hard to find a New England shoe factory that could list, among its assets, even $49.51 worth of laboratory equipment.

"But this failure to read the handwriting on the wall is by no means peculiar to New England. It is still, with a few conspicuous exceptions, characteristic of American industry as a whole. . . . In a situation so clear to us as chemists and chemical engineers, and so charged with peril to American industry, it is our imperative duty to translate the handwriting on the wall to those who mistake it for a mural decoration. . . . Let us . . . endeavor to educate the manufacturer to realize the opportunities before him, and let us teach the investor to appreciate the perils that confront those companies which ignore research."

Of the second address, "The Fifth Estate," I shall treat in a moment. It should first be said that Little believes that the cause of research is making headway, even if slowly, against the blindness of the manufacturer. He feels that bankers are more willing to listen to technical men, that science is constantly forging ahead. Young men contemplating research, he is confident, will find a place not only in the college laboratory, but in industry as well. They will make at least as much money, he promises, as their classmates "who go in for selling bonds."

It was his address made before the Franklin Institute on the occasion of its centenary celebration,

and called "The Fifth Estate," which won for Little undisputed eminence as the voice of research. Though made four years ago, it is still widely read. He is constantly called upon for reprints. In this speech, Little recalled the remark of Edmund Burke declaring journalists to be members of a Fourth Estate, more powerful than the lords spiritual, the lords temporal, or the commons. The Fifth Estate, "is composed of those having the simplicity to wonder, the ability to question, the power to generalize, the capacity to apply." Little went on to say:

"It is, in short, the company of thinkers, workers, expounders, and practitioners upon whom the world is absolutely dependent for the preservation and advancement of that organized knowledge which we call Science. It is their seeing eye that discloses, as Carlyle said, 'the inner harmony of things; what Nature meant.' It is they who bring the power and the fruits of knowledge to the multitude who are content to go through life without thinking and without questioning, who accept fire and the hatching of an egg, the attraction of a feather by a bit of amber, and the stars in their courses as a fish accepts the ocean."

One paragraph of "The Fifth Estate" establishes Little as a man of deep emotion, a scholar of rare learning, a man gifted with eloquence. In discussing W. R. Whitney of the General Electric, I have used Whitney's own words praising Langmuir to de-

scribe Whitney himself. The impulse to follow the same procedure here cannot be resisted. Little's tribute to Franklin might be, with the addition of a word here and there, an unconscious tribute to Little himself, a man of many parts. He said:

"Benjamin Franklin was not perhaps in all respects a paragon, but he was unquestionably a polygon—a plain figure with many sides and angles. . . . He was craftsman and tradesman, philosopher and publicist; diplomat, statesman, and patriot. And he was, withal, a very human being. What concerns us particularly on this occasion is the fact that he was at once philosopher and man of affairs. His remarkable career should refute forever the fallacy, which, unfortunately, is still current, that the man of science is temperamentally unfitted for the practical business of life."

Little, too, is a "philosopher and man of affairs." He, too, is many-sided. He, too, refutes the theory that men of science cannot make themselves heard along with the journalists, the bankers, the politicians, and the lawyers.

CHAPTER TEN

PAPER WORK

Hugh K. Moore

THE story of Hugh K. Moore might well be called "Helping Himself," "Making His Way," "Bound to Win," or "Rough and Ready." One suspects, in listening to his own narrative of his early days, that Moore would not object violently to any of these titles or, in fact, to very many among the hundred more of the same variety devised by the lamented Horatio Alger, Jr. Now wealthy, successful, the research director of one of the largest paper companies on earth, Moore looks down from his eminence on the years of struggle and privation. He finds satisfaction in what he sees, a somewhat detached and impersonal satisfaction which absolves him of the sin so common among self-made men, pride in their creator.

The moral he draws from his life is not the usual bromidic one of "work hard, be honest, be as intelligent as your quota of brains will permit." He likes to think that a determination to be independent, in thought and action, is the rule for success. This,

Hugh K Moore

no doubt, has been the secret of his achievements in the laboratories of the Brown Paper Company, at Berlin, N. H. But it penetrates his political beliefs and his attitude toward his family. A daughter, for instance, is studying at Radcliffe, and Moore, a typically proud parent, seems to delight chiefly in the determination with which she chooses her own courses and does her own work.

"She does what she likes," he boasts. "She's independent."

Closest to the Alger motif in the story of Moore is an almost tragic chapter, when he was thirty years old. He had supported himself at the Massachusetts Institute of Technology for three years and then, his money gone, had taken a job at Rumford Falls, Maine, in an electrochemical plant. While there he had invented a form of electrolytic cell and had obtained enough financial backing to start a small factory. Associates ran the plant, and Moore scurried around New England attempting to obtain orders for his cell. Occasionally he would rush back to solve some production problem and then would be off again. Prior to all this he had spent three years in perfecting the cell and during that time he had received nothing from his financial guarantors except his board and lodging. But he had, in the meanwhile, been married. The company began operations in 1900 and by 1902 it was on the rocks. Moore, absolutely penniless, had to find a job and,

an apparently impossible thing, to find $800 additional. His wife was gravely ill and doctors said that specialists must be called for a delicate operation.

Moore went down to Berlin, N. H., to see T. P. Burgess of the Burgess Mills to whom he had sold an electrolytic cell. He was, he announced, hard pressed for work. The factory had failed and he needed $800 for his wife's operation without delay. What could he do?

"I'll lend you the $800," Burgess said unexpectedly. "See my superintendent about a job."

The superintendent, however, was less cordial. Burgess, chief owner of the company and its treasurer, lived in Boston and did not concern himself with the details of operation. Moore was informed that the only opening was in the yards. The pay would be $1.50 a day. Having placed his wife in the hospital, Moore went to work and it proved almost unbelievably arduous. He was not a brawny person, had never done any really heavy physical work, and the men in the yards took advantage of his comparative softness. When a log had to be lifted Moore got the heavy end. He was maneuvered into doing far more than his share and so great was the strain that for the first month he was unable to work more than four days each week. At night—Moore still groans with the recollection of it—he would stagger to bed with his back aching so that

he could barely move, his hands torn, his arms burning. And yet, half dead with fatigue, he would set his alarm clock for midnight. At that hour he wearily pulled himself up, got into his clothes, and went over to the mill which ran a night shift. With a small notebook he wandered through the various parts of the plant looking for mistakes in operation. He found many and in a few weeks he buttonholed Burgess, at Berlin for a visit, to report that the mill was wasting nineteen tons of pulp a day. There were other operating losses which Moore, a trained chemist, believed he could reduce. Burgess told him to go ahead and draw up the plans.

"I said I could do it in two weeks," Moore recalls, "but I wasn't quite as good as I thought I was. It took me three, and Burgess was probably beginning to think I had been faking. When I finally finished, however, Burgess saw the value of what I had done. So he took me out of the yards and gave me a job as chemist at $75 a month. For several years he gave me a raise and each year I also got seven or eight bonus checks of $100 each. Meanwhile I had paid all of my wife's hospital bills and I went to Burgess to pay back the $800 he had loaned me. The first attempt I made was after I had been working for him about a year.

"He wouldn't take the money, saying that I could not yet afford it. Finally I wrote him a letter, to Boston, saying that I wanted to give him the money.

He wrote back—a pretty brusque letter, too—telling me that if I didn't stop trying to pay the $800 he would fire me. He said I'd saved far more than that in wastes at the mill. He *would* have fired me, too. He was just that queer. So I forgot about it."

From the work of those early days developed the research laboratory which Moore now commands and in which about 100 research men are employed. Its studies have given the Brown Paper Company, which took over the Burgess mills, a preëminent position in paper manufacturing, particularly in the manufacture of kraft paper. The work of Moore is an unanswerable reply to those who believe that old, established industries have no need to spend money on research. And yet, although he has gone further into the unexplored regions of fundamental research in paper making than many of his fellow industrial research directors; Moore looks exactly like a man employed in a paper mill. There is little suavity, of manner or appearance, about him. His hair—what there is of it—seems always to be slightly disordered. His eyes look out from behind tortoise-shell eye-glasses in a somewhat globular manner. In short, Moore looks out of place in a glittering hotel lobby, seated in the lounge of the Chemists' Club in New York, conferring with some of his dapper, well-dressed brethren of the world of research. But when he speaks they listen attentively, for his knowledge

is sound, comprehensive, fundamental. Although he is a practical paper man as well as a scientist, Moore is incapable of expressing his ideas in non-technical language. He is unhappy unless, when he is writing a paper, he can insert a mathematical formula on every other page. He believes that the lay public can understand what he is attempting to say as long as he limits himself to words of three or four syllables.

Looking back on Moore's early career and his association with Burgess, one is struck by the thought that two such independent men must have had difficulty in getting on in harmony. That they did so was due, one suspects, to mutual admiration. Moore felt that Burgess was an excellent financial man, a first-rate industrialist. The mill owner knew that he had found a valuable asset in an expert, original, industrious chemist. Moore's impulses toward independence started at an early age. The son of a Maine Congregationalist minister, who moved to Massachusetts later on, he was informed when a boy that a legal career had been chosen for him. He had, however, already been experimenting in a homemade laboratory and told his father that he intended to become a scientist.

"Go ahead," said his stern parent, in effect, "but don't expect me to put you through college. You'll have to make your own way."

This does not seem to have disturbed the son. At

all events, he continued to be independent, continued to be pugnaciously argumentative in his outlook. This tendency led him, inevitably, into scholastic debating, and since economics was another boyhood hobby he became almost devastating on the platform. He usually "took the other side," Moore recalls, and did so "out of obstinacy." In one debate while he was at high school the subject was prohibition, still a horrid improbability as far as the nation was concerned. Our hero argued that Federal prohibition would be a great mistake and grew prophetic in his arguments. The climax of his plea consisted of the production of several bottles of whiskey which he had made in his own laboratory. Such whiskey, he said, would be manufactured in the event of prohibition and it was, he told his audience, decidedly inferior in quality. To-day, fifty-six years old, Moore is more convinced than ever that prohibition is a mistake. But he does not drink.

By the winter of 1910 Moore's position in the paper industry was unassailable. Behind him stretched long years of privation, the years when he had supported himself at M.I.T., when he had worked as a laborer for Burgess, when he had gradually won distinction as a chemist. The Burgess company had been absorbed by the Brown Paper Company, and Moore's title was Chief Chemical Engineer. Great advances had been made in the chemistry of paper manufacturing and for many of these

Moore was responsible. He perfected a process whereby, in the manufacture of the wood pulp, an acid could be produced with the result that far less of the fiber was destroyed. He installed a complete chemical control in the bleaching process and was thus able to predetermine the color of the resulting paper. He then made major contributions to the work of eliminating dirt specks in the finished paper. But Moore's greatest triumph—an achievement which brought him fame and respect wherever paper is made—followed an order during the winter of 1910 to improve the operating efficiency of the company's mill at La Tuque in Canada.

A first principle in the paper industry is that by-products should be developed from wastes. Revenue derived from by-products reduces the cost of the primary product. Unless this is done, any paper mill is more likely to be recorded, on the ledgers of the company which operates it, in red ink. The officials of the Brown Paper Company had been worrying for more than a year over the new sulphate pulp mill at La Tuque. Its equipment and methods of operation were the equal of any plant in the world, but the weekly and monthly charts demonstrated that it was losing money. Reports from the superintendent told of frequent explosions which had sent many employees to the hospital. The method being followed for the recovery of the soda salts used in separating the cellulose from the wood

might not have caused high labor turnover among stolid, unemotional Norwegian or Swedish workmen, but at La Tuque Frenchmen were employed. Many of the workmen remained for only a week or so and then, terrified by some explosion, would quit their jobs. The necessity of hiring new, green hands further increased the probability of accidents. And the soda was not being recovered from the black, dense liquors which remained after the pulp had been made.

"Without the recovery of those salts," Moore was told, "it's impossible for the plant to pay. Jump up there and see what you can do."

Moore jumped—into a country still crude, uncivilized, without hotels or lodging houses, and bitterly cold. He arrived, with an assistant or two, to find that the spur-line railroad had not yet been extended to the mill and that the ground was covered with several feet of snow. All that winter he lived on snowshoes, making the trip from the railroad siding each morning and coming back, often long after midnight, to a freight car which had been fitted up as a crude room. But Moore was too busy to worry about discomforts. He faced a technical research problem which would have baffled many a chemist working in a warm, sunny, well-equipped laboratory. He had to assemble information hitherto unavailable. He had to work with the utmost possible speed because the mill was monthly eating

further into the profits of the Brown Paper Company.

What he had to do is best explained, perhaps, by a very brief account of some of the chemical processes of paper manufacture. Wood is the basic raw material from which paper is made and the valuable part of the wood is the pulp, or cellulose. In order to obtain the pulp, the wood is cooked with sodium compounds in a digester. This dissolves the lignin from the wood and leaves the cellulose. The solution of lignin and soda salts is called "black liquor" because of its dark color. Unless these salts can be recovered from the liquor the pulp cannot be sold at a sufficiently low price to make it marketable. But the method used at La Tuque resulted in repeated explosions, a heavy labor turnover, and monthly losses. The research problem faced by Moore was to ascertain the causes of the explosions and either eliminate them or evolve a process which did not involve the same risks.

A chemist has, normally, no difficulty evaporating liquids. But the so-called "black liquor" lived up to its name in being balky, objectionable, hard to handle, possessed by devils. It acted in just the way carbolic acid and formaldehyde had acted when L. H. Baekeland was seeking the phenol resinoid later to be known as Bakelite. When the chemist tried to evaporate it, "it foamed most abominably," as Moore has described it. In the second place, the

compounds had properties which made it difficult for the engineers to design machinery. The usual method of evaporation was by means of disk evaporators consisting of a number of disks attached to a revolving shaft. The disks were so arranged that on the bottom of the shaft they were immersed in the liquor while heat, usually waste heat from the plant boilers, passed by the upper disks above the pool of liquor. As the disks revolved they picked up a film of liquor which was then evaporated by the hot boiler gases. Then it was guided into a rotary drum, or incinerator, through which flames were passed. This removed such water as remained and charred the organic matter, largely lignin. Then the soda salt mixture could be handled in a recovery furnace.

The explosions occurred after the liquor had been passed from the disk evaporator to the incinerator. The black liquor contained queer lumps, sticky and tending to adhere to each other, which had made it impossible to handle, and the purpose of the incinerator was to char these lumps. Unfortunately there were wide variations in the concentration of the liquor. At times there would be rings of partially charred "black liquor" in the incinerator and a pool of white hot alkali would be separated from the remaining liquor. This ring would break and the liquor would pour over the molten alkali. Then would come the explosion, with steam and flying

liquid filling the air. In its train, a mixture of profanity and yells of pain.

Moore decided that the first thing to do was to obtain all possible information on the nature of this liquor. He wanted to know its boiling point, specific gravity, latent heat of fusion, and a dozen other things. He dispatched an assistant from the wilds of Canada to the library of the Massachusetts Institute of Technology with orders to obtain innumerable physical and chemical constants of the substances involved. Others, never previously determined, were worked out in the crude laboratory at La Tuque. Then the assistant at M.I.T. was instructed to obtain all possible information regarding Stephan's law of radiant heat—heat in a form which will pass through an atmosphere without causing heat as it passes. On the basis of Stephan's law, Moore worked out an entirely new method of evaporation. He designed entirely new machines and had everything under absolute control.

Moore worked for more than two years perfecting his new process. In effect, he eliminated the harmful and dangerous explosions by substituting explosions which could be controlled and were, therefore, harmless. What he did was to design apparatus in which the thick, black, viscous liquor is pumped to a vessel and maintained under high pressure and high temperature until it can be projected as a spray into a combustion chamber. There it explodes. The

water is evaporated and the organic matter is burned off. The resultant product contains all the valuable soda salts. The process eliminates much manual labor and, of course, all danger to the workmen. Moore required more than a bulldog tenacity in research to perfect his scheme. He needed calm, old-fashioned, personal courage in the face of real danger. At one stage in the experiment a boiler had to be erected. Every one at La Tuque was convinced that it would explode, and kept at a safe distance. Finally, when nothing happened, they ventured near it. And gradually the management of the pulp mill became convinced that Moore knew what he was doing. He admits now, however, that he started the boiler going with mixed emotions and not without misgivings as to what might happen.

Moore accomplished his purpose, the recovery of the salts and the elimination of danger. In doing so he had run into many interrelated research problems. The job was to produce cellulose of paper, wood pulp. But from cellulose, such are the complications of research, are fashioned articles almost impossible to list. Behind all these are thousands of research problems. If the packing industry has been able to use everything but the pig's squeal, the paper industry has gone them one better. Using wood as raw material, the paper makers have saved everything including the bark. Nothing is wasted—with the result that to-day the public reaps another divi-

dend of research. But this is not part of Moore's story.

Moore has never forgotten those days in the bitter cold of Canada, nor has he forgotten that when the problem seemed utterly baffling he could make progress only by the acquisition of a detailed knowledge regarding the balky substances with which he was dealing. He has never forgotten that long and costly weeks were lost while assistants worked in the M.I.T. library and in his own laboratory to determine the chemical and physical constants of these substances. He was, consequently, an enthusiastic supporter, some years ago, when at the suggestion of the International Research Council the National Research Council of the United States undertook the enormous task of preparing "International Tables of Physical and Chemical Constants." Through coöperative research by scientists throughout the world the physical and chemical constants of every known substance were to be determined. The project was known to be expensive, and Moore, whose own eminence had a direct relation to fundamental data, gladly agreed to assist in raising the money. At a time in the early stages when the project might have failed from lack of initial encouragement, Moore obtained pratically unassisted a substantial sum towards the expenses of the work. Six volumes of tables have been completed, and the seventh started.

Moore still lives at Berlin, N. H., where, so many years ago, he arrived with a critically ill wife, penniless and jobless, and needing $800 immediately. He is an astonishingly versatile person for one whose life has been spent amid the technicalities of chemical research. In 1912, for instance, he was a delegate to the state Bull Moose convention. In 1922 he ran for the state legislature because he knew that some evil legislation was to be forced through and was, to the surprise of every one, elected. He had a magnificent time at the capitol blocking bills which he believed bad, making speeches, making himself obnoxious to the machine politicians. He won most of his fights and returned to his laboratory, like Cincinnatus to the farm. But his interest in politics continues a vital part of his inclination toward debating and independence of thought. Economics is his hobby and the extent to which Moore differs from those scientists who are incapable of keeping their check books balanced, at a loss in the world of practical things, was further illustrated a few years ago.

One night, a delegation consisting of two or three of Berlin's leading citizens called at his home. One of the village banks, they told him, was having financial trouble. It was, in fact, very nearly on the rocks and if it crashed, the outlook was ominous. Many of the depositors were employees of the Brown Paper Company. There would be widespread re-

sentment, even possible labor trouble. A large proportion of the workers were foreigners with little understanding of English. Riots might result.

So Moore, the chemist, took over the bank. Within a compartively short time its credit had been reëstablished. To-day it is paying dividends. He is confident that he is an excellent economist and not a bad financier, and the people of Berlin, N. H., are unanimous in agreeing with him. Hobbies aside, Moore considers but one thing important, the accumulation of scientific knowledge. He has long been threatening to resign as director of the company's laboratory and he now insists that he will do this in four years. He will then get to work in a laboratory of his own "to look into some things that interest me." He also intends to write some books but these, it is safe to predict, will be on no best-seller lists. In them, Moore will make no effort to write nontechnically. He will revel in formulæ and long words. They will probably be unintelligible except to his learned brethren—the members of "The Fifth Estate."

CHAPTER ELEVEN

A FISH STORY

Harden F. Taylor

"Science," said Dr. Arthur D. Little of Cambridge, Mass., in a recent address before the Franklin Institute, has "disclosed the stupendous complexity of simple things." To phrase it in another way, science has been neither aloof or snobbish. It has worked to produce a perfect loaf of bread, coffee brewed by scientific formulæ, a rust-resisting kitchen knife. It has willingly given its talents to the everyday things of life. And because this is so housewives in Albuquerque, N. M., and Pueblo, Colo., can now obtain from the corner grocer or butcher frozen fish which, chemically and by the common test of taste, is identical with that just caught by fishermen tossing in smacks off the Grand Banks. A man with imagination peered, for many weary hours, through the lens of a microscope—and the tiny cells structure of fish meat became as large and as familiar as the steel framework of a skyscraper. Intimate knowledge of the physical and chemical changes going on among and within the microscopic cells was turned

A. H. Taylor

to practical advantage and was used in laying the foundation for new prosperity in the fisheries industry. The man behind the microscope has been made a vice president of one of the largest companies and is dragged from his research laboratory, from time to time, to sit in the councils of Big Business.

It was at about the turn of the century that the son of a Methodist minister, living in various parts of North Carolina as his father's pulpit was shifted from town to town, was passing through a phase common to most small boys; the science phase. He was an animated question mark. What made the winds blow? Why was coal black and snow white? What would happen if the ammonia used for washing windows were mixed with vinegar? He rigged up apparatus in the bathroom and brought down parental wrath by filling the pastorate with horrible odors on the afternoon that his mother was serving tea to the Ladies' Aid. The hired girl predicted that there would be an explosion and threatened to give notice. Unlike most youths making elementary science a hobby, however, Harden F. Taylor did not transfer his interest to something else; science has been both vocation and avocation to him ever since. Yet it was the accidental acceptance of a summer job in the United States Bureau of Fisheries which caused him to become, before three decades had passed, Vice President for Scientific Research of the Atlantic Coast Fisheries, of New York, and to re-

organize an industry with the aid of science. To-day, as a result of his studies and under his supervision, carloads of frozen fish fillets are shipped daily to all parts of the country. Heads and tails, skin and bones, have been removed and each fillet (or steak) has been wrapped by machine in a neat, attractive package. The inedible portions are used as a by-product and the profits resulting from them make up the cost of refrigeration and research. And the housewife saves money because she does not have to pay shipping charges for portions of the fish which, at best, can be fed only to the cat.

During 1911 Taylor was an undergraduate assistant in biology at Trinity College (now, by the grace of tobacco millions, Duke University) at Durham, N. C. He was then in his junior year and was just under twenty-one years old. His duties consisted largely of capturing tadpoles and frogs for use in the laboratories, but occasionally he had an opportunity to attempt a few simple research problems under the direction of the professors in the department. Being the son of a minister, Taylor was always cramped for funds and he eagerly accepted, therefore, an offer to become a junior scientific assistant at the Beaufort, N. C., experimental station of the Bureau of Fisheries. It was the custom, at that time, for university professors to engage in research at Beaufort during the summer, and Taylor supposed that he would again catch tadpoles, frogs,

and fish. He was astonished and a little disconcerted upon arrival, then, to be assured that facilities had been provided for work of his own.

"They asked me," he has recalled, "what problem I intended to work upon; apparently one of the professors at Trinity had told them that I was qualified to do some original work. As a matter of fact, I had no problem and for several minutes I could think of no answer. Then I told them, somewhat vaguely, that I longed to experiment with shellfish. This seemed to satisfy them, to my surprise, and during the summer I collected specimens, studied them, and photographed them by means of a new color process. It was a pleasant and profitable summer and I went back to the station in 1912, 1913, and 1914. In this way I became interested in fish. In later years I was destined, on some occasions, to regret it. It seemed to me that I had chosen a scientific path leading nowhere. The fisheries industry was having a hard time making profits, and most of the men in it were disinclined to accept advice from specialists whose knowledge they considered theoretical."

Taylor is still young enough, being under forty, to recall his early years with great clarity and he is human enough to derive a mild degree of pleasure from contemplating his swift rise to distinction. While in New York he divides his time between the offices of the Atlantic Coast Fisheries situated, as

befits the headquarters of a new Big Business, in the financial district and the research laboratories which he commands. These are on the top floor of an old building, squatting under the Brooklyn Bridge on Water Street, which was the first factory from which fish frozen under Taylor's process was shipped. The structure is but a short distance from the ancient Fulton Fish Market where, as a boy, Alfred E. Smith held his first important job. It is now used only for research purposes; the present factories being located at New London, Conn.

Like most of the other later-day scientists, Taylor looks not in the least like the popular conception of a man who spends his working hours adjusting the lenses of microscopes, making delicate measurements that call for steady hands and steadier nerves, studying the chemical structure of fish meat. He is, of course, unusually young for the post he holds, being only thirty-eight. On first appearance he might be a broker, a rising young attorney, a banker. He is of medium height and rather slender. His eyes look at the visitor steadily and in a friendly fashion from behind rimless glasses. That he uses them more than most men is indicated by deepening furrows between his eyebrows.

He violates another popular theory in his ability to talk forcefully, clearly, and in nontechnical terms concerning the work he has done and his hopes for the future. He seems, in short, to be a man very cer-

tain of his standing in the profession he has chosen, aware of his abilities, fully qualified for the problems he will face. Yet in his laboratory, wearing a white coat and absorbed in some experiment, it is obvious that he belongs to the ancient fraternity of those who labor to reach that remote realm where dwell facts. Then he alternates between exultation, as the experiment at hand goes well, and momentary gloom as the flame of his Bunsen burner shows red when it should gleam blue or purple, or when some precipitate in the bottom of his test tube fails to meet expectations. Then he works late into the night, forgets to come home for dinner, forgets to keep his engagements, ignores requests from high officials of his company that he attend important meetings.

The story of Harden Taylor cannot be classed with the conventional "success story" in which a youth bent upon fortune labors arduously, studies hard, watches for opportunity, and wins fame and fortune through some carefully worked out plan. Most of these stories are largely untrue, any way. It was, we have seen, a casual summer job which introduced him to the technical mysteries of fish. And even after he had spent three summers studying their peculiarities he had no urge to make this his life work. He taught school, as a matter of fact, during the first two years after graduation from college. His brief career as a teacher is interesting,

however, because of the extent to which it explains the man's mind. Even then still in his early twenties and more boyish in appearance than some of his students, a restless spirit, he had an inquiring nature which compelled him to seek new ways of doing established things. It was an outcropping of the pioneer spirit which a few years later marked him as a research worker. And his experiments were the more unusual in that he attempted them in North Carolina, in those days anæsthetic to new ideas. Two teaching jobs were available for the autumn of 1914, one at a high school with a fully equipped laboratory and well-organized science department and the other at a small village school at Tarboro, N. C., where science was still viewed as among the black arts. Taylor took the second job, although the pay was less. He convinced his superiors of the value of his ideas, built a laboratory and began to teach his subject according to highly original theories. These, it might be added, have since been adopted by some of the most modern experimental schools in New York, although no one is aware that Taylor did some pedagogical pioneering. Taylor felt that high school science had been made too formal, that students were told a vast variety of things and expected to remember them whether they understood them or not. Instead of following this method, he devoted many weeks, even half of the term, to one experiment in chemistry, physics or

biology. He saw to it that his boys and girls had a fundamental understanding of the subject. He gave no examinations, as examinations are commonly given; he permitted his students to work on some problem which required original thought. Again the philosophy of the research man is evident!

"It worked very well," he remembers with gratification. "You could hardly get those kids out of the laboratory at night. They worked until it was dark."

Undoubtedly Taylor was a good teacher and would have adorned that profession. But officials of the Bureau of Fisheries could not forget that he had shown unusual promise during his summers at Beaufort and in 1915 they persuaded him to take a civil service examination for the post of scientific assistant. He came out at the top of the list and that fall, just after his twenty-fifth birthday, he was appointed to the Division of Scientific Inquiry with his central headquarters at Washington. He began a long series of investigations relating to fisheries; his was no swivel-chair job. He studied pollution of waters, taking specimens from evil-smelling rivers and streams and analyzing them in his laboratory. He had not been on the job long when the Bureau of Fisheries received frantic appeals from fishermen operating on the Gulf of Mexico. Fish in those waters were dying by the thousand, they said. What was the matter? Taylor went to the scene, set up a portable laboratory, and went to work. But on this

occasion he could find no explanation; the condition of the water was about normal, there seemed to be no epidemic of disease. Taylor's only theory was that there had been disturbances under the bed of the sea which had killed enough fish to feed the state of Florida for a year. Another time he was detailed to make what biologists call "a life study" of the shad. He followed the shad in its migrations from Florida to Canada. As a result of this investigation he reached the conclusion that the shad, once plentiful, cannot long survive modern conditions. Factories are poisoning the rivers of the nation. It is only a question of time before the shad will be extinct.

Then came the entrance of the United States into the World War. Taylor, working in the Bureau of Fisheries, could not see that his activities were adding to the prospect of an Allied Victory and he attempted to enlist. However, the possibility of a food shortage already loomed, and he was ordered to remain where he was. The specific reason for the order became clear when, a few months later, President Wilson set aside $125,000 to be used by the Bureau of Fisheries for the construction of a research laboratory in which commercial processes relating to capture, preservation, and distribution of fish were to be studied. This gave Taylor his first big job. He was by then enjoying the cumbersome title of Assistant for Developing Fisheries and Saving and Use of Fishery Products. He was instructed to

travel throughout the country investigating other laboratories and to purchase such equipment as, in his opinion, would be most valuable. For a month or so he did this, comparing the methods of one research laboratory with another, studying organization and methods and costs. He discovered that the laboratories of the meat packers at Chicago were working along the most similar lines and he spent some time conferring with the scientists employed there.

Whatever criticism may be made of the Wilson Administration's conduct of the war, it cannot be charged that the officials were optimistic regarding an early peace. They proceeded on the assumption that the struggle would be a long one; far longer, in fact, than it turned out to be. And one of their concerns was the maintenance of a food supply. Whether Herbert Hoover, in general command, conceived the idea of the new laboratory is not known. But the obvious purpose was to make certain that the oceans and the rivers could do their share, if necessary, in feeding the nation and the armies in France. And the value of fish as food was being neglected.

"Warm-blooded land animals," Mr. Taylor has explained, "are notoriously wasteful of their food energy, for the greater part of it goes to keep their bodies warmer than their surroundings. About three-fourths of a man's food, for example, goes to keeping his body warm and vital functions going;

only about one-fourth being available for thinking, acting, and growing. The cold-blooded animals of the seas, by virtue of being excused from the duty of keeping themselves warm, use a greater portion of their food in growing. For this reason, fish must be much more efficient manufacturers of food than land animals and they draw upon a vastly greater supply of natural energy, sunlight.

"With the human population of the earth rapidly increasing, the Old World continents already saturated and overflowing; with frontier countries already opened up and even being rapidly industrialized; with civilization everywhere encroaching upon agricultural and pasture lands, we cannot much longer neglect the ocean as a great producer of food, to say nothing of its other products of value."

The war ended before any real food shortage developed. And in late 1918, Taylor remarks, "the fish industry was virtually for sale." To understand the reasons for this it is only necessary to remember the old-style fish store, a type which has by no means disappeared as yet. It was a place filled with reeking, dirty, canvas-covered barrels half filled with ice and the anæmic carcasses of what had once been glistening salt-sea fish. The fishing industry had dropped far behind in the race to make its product attractive to the consumer. Bread was being delivered in clean, sanitary packages. So was butter, and many forms of meat, and a score of other house-

hold products. But fish were being handled in such a manner that they rarely reached the retail market in first-class condition. Few except small dealers would handle fish; so odorous and disagreeable were they. The average butcher and nearly all grocers declined to have anything to do with fish.

Such was the problem faced by the fish industry and solved, in due time, by Taylor. He was confident that the future depended upon getting goods to the consumer in a fresh, sanitary condition. The equipment of the new laboratory at Washington was called into use and the work began.

What were the technical aspects of the problem? Taylor's mind went back to the drowsy summer days at Beaufort, N. C.; to his first experiments in the physiology of fish. He recalled a lecture in elementary biology in which some professor had explained that fish were "cold-blooded" organisms. Their body heat was well below that of animals living on land. It was for this reason that fish spoiled more easily. Then, too, the tissues of fish are tender in the extreme. They need no strong, tough ligaments to float in the water nor do they require resistant skins. As Taylor pondered this he thought, again, of the accepted methods of packing fish; clumsy barrels half filled with ice and with the fish jammed down into them. No wonder they reached the market in anything but an appetizing condition!

Bending over his test tubes, his scales, and his ther-

mometers at the Bureau of Fisheries, Taylor concluded that the scientific approach to the problem of arresting deterioration of fish lay in the physiological chemistry of the living fish. With this he was already familiar, of course. He knew that there were many chemical reactions—the digestion of food by gland secretion, muscle contractions, oxidations, excretion of end products, synthesis of complex substances, and the breaking down of these into simpler ones. All these go on more or less continuously and with scientific nicety, presided over and regulated by the nervous system and the glands of internal secretion.

But when the fish is caught and dies, Taylor knew, when the nervous system loses control, and respiration and heartbeat cease, changes of a disorderly and destructive sort begin to run riot. An example of this is found in what biologists call "autolysis." In every tissue of the living fish is an enzyme capable of digesting and liquefying that tissue. By this means nature enables the animal to live at the expense of its own body in the event that other food is lacking. Normally, these chemicals are held in check, but when the fish dies they become active. Heat is produced. Bacteria are set to work. And the meat takes on undesirable flavors. It was necessary, therefore, for Taylor and his associates to stop this process known as autolysis.

"The one great enemy of these destructive pro-

cesses," Taylor explains, "is low temperature. Cold is the king of all preservatives. Refrigeration is the withdrawal of that which is necessary for decomposition—warmth. It adds nothing whatever to the thing preserved. Refrigeration can be applied at will, maintained indefinitely, controlled with precision, and withdrawn at any time, the things preserved being thereby restored to their original fresh condition. A moderately low temperature retards both autolysis and bacterial decomposition. If sufficiently low temperatures are applied, both are absolutely arrested. But how low is sufficiently low?"

This was the question Taylor proceeded to answer in the Bureau of Fisheries laboratory. Bear in mind that his study was strictly practical. Such, indeed, was the basic purpose of the $125,000 laboratory. What could the government do to assist the imperiled fish industry and provide fresh, palatable fish for public consumption? Taylor and his assistants knew that cold was the answer to deterioration. Should fish be frozen? This seemed logical and yet there was wide public prejudice against the accepted method of freezing. And it developed that the prejudice was not based merely on a whim, as is sometimes the case, but on the sound fact that frozen fish did not compare in palatableness with fresh fish.

"Does freezing impair the edible and dietetic qualities of fish?" Taylor asked himself. "If so, in what way?"

He again turned to his microscopes. Freezing, of course, is a change of state from liquid to solid. Water, like most other simple substances assumes a crystalline form in the solid state. The traditional method of freezing fish had been to place them in a cold room with a temperature of about zero. Taylor examined the cell structure of fish meat so frozen and found that large crystals had been formed and that these had ruptured the delicate membranes. Upon thawing, he discovered, the juices normally imprisoned ran out. A mere ghost of the fish remained; meat robbed of its flavor; fiber with its best substance lost!

Taylor knew, as do all chemists and physicists, that the size of the crystals formed by freezing depends on the rapidity of the process. Freezing a fish at zero takes about twenty-four hours. Would the cell destruction be less serious if a quicker process were substituted, if lower temperature were used? It was worth trying. For many weeks he worked with his microscope in temperatures that ranged from zero to twenty degrees below zero. He could do so for only a short time, so intense was the cold. But he was rewarded for his efforts. Fish frozen at from 15 to 25 degrees below zero showed microscopic water crystals, but they were *inside the cells* and muscle fibers. The membranes were not torn. When the fish is restored to warmth there is no loss of valuable juices. The water—and fish consists of from

75 to 80 per cent water—is reabsorbed by the protoplasm. The fish, chemically and as to taste, is exactly the same as it was before the freezing process started.

So much for the laboratory discovery. Taylor had been hard at work on this and allied problems for five years and, although he was not yet thirty-five, he had become Chief Technologist and then Chief of the Division of Fishery Industries. This means that he was probably the best informed man in the country on the technical perplexities of the fisheries industry. But he was not, in 1923, entirely satisfied with what he had accomplished. He had a lingering suspicion that some of his researches had been too theoretical. Anything, he knew, could be done in a laboratory. Could the new freezing process be adapted to large-scale production? At about this time he happened to meet Ira M. Cobe, a business man who had just accomplished the reorganization of the Atlantic Coast Fisheries and was chairman of its board of directors. The company was then far from prosperous and really consisted of four or five well-nigh defunct fishing corporations.

Cobe felt, as did Taylor, that rejuvenation of the industry depended upon getting its product to the consumer in palatable, fresh, attractive form. After the two men had talked together for an hour, they knew their viewpoints harmonized. Cobe was elated to find a scientist who could explain technical matters in plain, clear language. Taylor was pleased

that a business man was receptive to advice from a research worker. And he accepted an offer to become the director of a research program for the Atlantic Coast Fisheries.

Taylor's first laboratory was a small room in an old building on Water Street. There he perfected refrigerating machines in which fish could be frozen in large quantities and at temperatures which prevented the formation of large, tissue-rending crystals. It was not long before he had proved to Cobe and the other officials of the Atlantic Coast Fisheries that in his methods lay financial prosperity. At first, it is true, he was listened to with a degree of skepticism. But his experiments were soon accorded the greatest respect and in 1925 he was elected Vice President for Scientific Research. A large plant was erected at New London and soon its capacity was being trebled. The fish are unloaded from the company's fleet, cleaned by machinery, frozen and packed. A new type of refrigerator car has recently been adopted. Unlike those in general use, each contains a small refrigeration plant and temperatures approximating zero are obtained without difficulty. The new method of distribution has been so successful that chain grocery stores are now handling frozen fish.

Obviously, the elaborate freezing and packing processes are costly. Here, again, science demonstrated its capacity for common sense. Taylor pointed out

that the heads, tails, bones, and skin are of no use to the housewife. These are now removed before the freezing begins. Only the fillets are shipped and the by-products manufactured from what would be waste in the hands of the consumer pay for the freezing and other treatments.

It is unnecessary to remark that the laboratories directed by Taylor constitute a most important part of the Atlantic Coast Fisheries. And the scientist in charge might, if such were the nature of scientists, now concern himself only with minor problems that come up from time to time. But the man engaged in research is inherently a restless soul. After one experiment has been worked through to a successful conclusion he seeks new puzzles. After his scientific refrigeration had become an accomplished fact Taylor turned to improved methods of smoking fish, and here again it proved to be possible to improve not only the product but the process of smoking. To avoid the entanglements of old traditions and suppositions (which are frequently serious impediments to scientific progress), Taylor proceeded to scrap in their entirety the processes of smoking fish hitherto practiced, and to begin at the beginning, to apply smoke to fish to accomplish the desired purpose of imparting an agreeable flavor and, incidentally, a pleasing color. He found that smoke as usually applied has a serious drying effect on the fish and that, by taking advantage of the laws of psychro-

metry, smoke could be so applied that the original juices would remain in the fish. By studying the composition of wood and its products of decomposition, he found some highly useful facts: that smoke contains many chemical substances, some giving that pleasing smoky flavor, others being responsible for the color produced, still others for the preservative effect of smoke. Instead of trusting to luck, as smokers have done in the past, he proceeded to select the full wood so as to give the maximum of things wanted and the minimum of things not wanted, and to eliminate entirely by proper controls built in the apparatus certain long-named constituents of smoke whose presence is decidedly objectionable. Since our preferred preservative, cold is available, and we do not need the preservative in smoke, which toughens the fish and actually destroys some of its most savory substances, he contrived a means of eliminating them altogether.

Having worked for a year on the theory—chemical and physical—of smoking, and laid out the principles, Taylor then designed an apparatus that might be called a smokehouse, but you would never suspect it to see it. A control cabinet with dials, gauges, valves, meters, switches, and rheostats, gongs, bells, and other signals. The fish are put in on suitable carriers, the controls set, switches closed and the apparatus runs itself—when a bell rings the batch is finished—in about an hour, as compared

with six to fifteen hours by old methods and a vastly improved product. By the old methods a skilled "old-timer" at smoking had to be employed, and when he was absent, the work had to stop. By Taylor's method, any operator can be shown how to set the controls and throw the switches—the brains and experience are built into the machine.

Not quite satisfied with perfect smoking, Taylor felt that something could yet be done to improve the flavor. He reasoned thus: The flavors that we like most are those which are found in our natural foods, while the strong spices and condiments are not among those that would be encountered by us in our natural foods. If a juicy steak or a fresh mackeral contains delicious flavors, these flavors arise from certain definite things in the steak or mackeral. Why not do some chemistry to the steak and mackerel, isolate and identify the savory substances, then produce them and add them to our smoked fish. These flavoring substances, far from being harmful, are themselves food, and are derivable from meats, grains, and fish. So Taylor obtained some of these substances and put them in the smoked fillets—out-naturing nature, and making a smoked fillet that is far more delicious than the fish as it comes from the water, and by means that are beyond reproach.

It is usually true that the public shares in the dividends of research. In Taylor's case, the consumer living far from the seaboard can obtain healthful,

palatable fish. The price is lower because there is no waste. But Taylor feels that the scientist whose research is being undertaken in behalf of some industry has an even greater responsibility than increasing the prosperity of his company and reducing the consumer's cost of living.

"He is called upon for more than mere loyalty to his employers," Taylor says. "He must be loyal to the public. The banker who may happen to be the head of a food products corporation does not appreciate the harmful effect of certain processes. The scientist working in the laboratory supported by that corporation knows. He knows when the public safety is jeopardized. He knows whether foods are pure or not; not merely by legal standards but by ethical and scientific standards. He betrays his profession if he permits the use of any process resulting in foods which are in any way injurious. Nor can he permit a food to be sold for something it is not. Adulteration and substitution must be repugnant to him."

William H. Bassett

CHAPTER TWELVE

"LITTLE GRAINS OF COPPER..."

William H. Bassett

It is, let us say, twenty years ago. Down at the Barge Office on the lower tip of Manhattan Island a group of customs inspectors, and a handful of ship news reporters, are gloomily playing poker. It is late in the afternoon and the S.S. *Ilderim* (a fictitious name is safer because ship's engineers are a crabby lot) is not yet at Quarantine. Her owners report that she has been delayed by leaky condenser tubes. These, fashioned from an alloy of copper, brass, and zinc, had been corroded by the action of the salt water used in condensers for cooling. They had been leaking, salt water got back into the boilers, and the Scotch engineer had developed new and purple profanity. The ship's engines had been slowed down. The S.S. *Ilderim* would be seven hours late into Quarantine.

To-day, however, the S.S. *Leviathan*, the *Aquitania*, the *Majestic* or any of the other marine aristocrats arrives in port with nearly the regularity of a limited train. Delays, unless heavy storms have

tossed the Atlantic, are virtually unheard of. Fog may cause the captain to order half or quarter speed. But he is rarely forced these days to limp along because of engine trouble. This is due, in part, to the fact that microscopes became an essential part of the mill equipment at the plants where brass and copper are transformed into condenser tubes. They no longer corrode. They no longer crack. And they rarely develop leaks. That the modern ocean liner slips into Quarantine "on time"—like the Broadway Limited—can be attributed to the perserverance and the scholarship of quiet, land-bred research chemists; men who become seasick when crossing a river by ferry. Men who, in some cases, had never heard of boilers, condenser tubes, and marine engines. But they were able to examine the microstructure of copper.

William H. Bassett, who led the research work, was looking for a job in 1902. He had done some teaching and had been employed by the New Jersey Zinc Company since 1891 when he had been graduated from the Massachusetts Institute of Technology—that laboratory brooder where so many future directors of research have been hatched. He had worked, there, with Charles L. Reese who later became research director for the Du Ponts and who was responsible for many of that company's startling developments with explosives. But the appearance of a new superintendent at the zinc company

made Bassett's future uncertain. He made inquiries regarding what he would be expected to do and was informed that, if he did not feel happy, he might just as well quit.

"I'm not ready, yet," he told the officials. "I'll look for a new place, though."

"All right," they answered, "Your work is satisfactory and you can stay as long as you like."

Bassett felt that there was slight enthusiasm in the company's attitude and he asked his former professors at M.I.T. whether they did not know of a post for a young and enthusiastic chemist. It chanced that the Coe Brass Company of Torrington, Conn., was about to constitute a part of the American Brass Company's merger and wanted a man. Bassett became its chemist. The manufacturers of brass were having numerous difficulties with their manufacturing processes and were anxious to learn more about copper and copper alloys. But there was very little earlier work on which they could rely. Studies had been made with ferrous metals and they were told that a microscope would be extremely useful. So the Coe Brass Company purchased a lens. The chemist whom Bassett was to replace reported, however, that the instrument was of small value and that nothing much could be done with it.

The New England brass and copper industry was, in 1900, just emerging from the guild, or trade system. Skilled workmen, whose fathers had been in

the same industry, had a rough familiarity with the processes. Every plant had its own secret way of doing things and it was considered the height of bad form for the owner of one mill to visit another plant, or, in fact, even to attempt to do so. It is interesting to note that, great as the changes have been in the past two decades, a shadow of this still hangs over the industry. The electrical research expert thinks nothing of dropping in at the laboratory of a rival plant. But copper and brass research men seldom visit each other. Bassett felt, in 1902, that there were great possibilities in brass and copper research. Having assumed his rôle as chemist for the Coe Brass Company, his first act was to visit Professor Sauveur at Harvard. Sauveur had recommended the purchase of the microscope and had been highly irritated by the decision of Bassett's predecessor that it was of no value. Bassett reassured him, however, and received much valuable advice on how to begin his work.

The Socratic rule of "Know thyself" is translated, by the research worker, into an equally important dogma, "Know thy material." The chemist whose task it is to improve a chemical process first learns everything that he can about the peculiarities of the substances with which he is working. Moore, of the Brown Paper Company, for instance, was laboring in the frigid cold of the North Canadian woods in an effort to halt explosions which occurred in efforts

to recover valuable salts in wood pulp. He had first to obtain detailed information about all the chemicals which took part in the process. Langmuir, working with gases in vacuum at the General Electric Laboratory, became thoroughly familiar with all the properties of those gases. So Bassett, beginning with copper, had to become thoroughly familiar with the properties of a metal which was, in 1902, only casually known "to the trade."

The suggestion that research be made into the microstructure of copper and its alloys had come from a large consumer of nonferrous metals. Why, this customer asked, can you not do with your product what Howe, Sauveur, and others had done with steel? Such was Bassett's job. He found that while there was some literature on the metallurgy of iron and steel, there was none on copper and its alloy. Sauveur had told Bassett that the methods whereby experimenters on steel were able to peer into the inner structure might be adapted to the work and the first step in the problem was to find an etching solution which would clearly define the grain structure of the metal. The chemicals that chewed out crystal structure of steel were found to be of no value. How could the grain structure, the crystal structure boundary, and the details of twinned crystals, characteristic of copper, be etched out in such a manner that the metal's internal panorama would be clear under the microscope? Oxidizing agents, such as nitric acid and

chromic acid, failed. Electrolytic etching gave the first hope of success, but it was extremely uneven. In attempting to etch the surface (only on parts of it had the necessary detail been given) Bassett found a process which would work. He suspended the sample in such a way that the electrolyte was held to its *under* surface by surface tension. Then hydrogen peroxide and ammonia were the basis of further experiments and these, because of their rapid action and ease of application, are now almost universally used as reagents for etching copper-zinc alloys. The electrolytic process is still necessary, though, for bringing out minute details. Particularly is this so when the alloy under investigation is rich in copper.

So was born the micrometallurgy of copper, brass, and the other useful combinations of the nonferrous metals. The microscope, bought in the faith that science could help industry, was dusted off. Bassett thrilled in his discoveries like a pioneer who enters a new land and discovers new mountains and green valleys. Hundreds of samples of metal were ground, polished, and etched in scientific eagerness. Skillful fingers transferred to paper the new configurations of the metal that eyes saw. Systematically the ingredients of the metal melts were varied, first more copper, then more zinc. Then a dash of nickel. Now a small addition of tin. Copper was hammered, teased, heated, and chilled. All the mal-

treatments of the mills and foundry were inflicted upon the metal samples in the experimental spirit of finding out what goes on inside the hard surface.

Out in the mills where the actual metal was produced, where men wore rough shirts and were blackened with their daily toil, where father handed down to son the secrets of how to manipulate the rolls and furnaces in order to produce the wire, sheets, and tubing that orders required, in the actual workshops, there may have been secret scoffing at the white-collared chemist who thought he could tell them something by squinting down the barrel of a little microscope. As though metal were filled with germs! Ha! others had tried such tricks and still the old order reigned. It takes a brass worker to make brass.

Such may have been the first reaction to Bassett's early ventures into the submicroscopic realms of metals. New ideas and new things always must be pried into human minds. Those who cut wheat with cradles burned the first reapers. Hand weavers thought the first semi-automatic looms were creations of a malevolent providence and administered the proper punishment.

But the magic of Bassett's microscope convinced the practical men on the mill floor. They may have come skeptical and prejudiced, but the new aspects of a metal with which they had worked for years and yet had never known turned any unspoken antagon-

ism to coöperation. Bassett let the men look inside the copper and brass. For the first time since some ancient inquisitive half-savage discovered copper's usefulness some ten thousand years ago, those who fashioned the metal into industrial tools were able to see the crystals which are the essence of the metal.

The case-hardened mill men saw that Bassett's work would help them. Early in the investigations Bassett was able to tell the why of some trade secrets. A man bending over a microscope could tell, by noting the form of the crystals, which metal had been rolled, which metal was hard and which was soft. The intimate examination of the metal's structure showed why the practical method of governing the annealing or heat treatment by surface color allowed spoilage and irregularities to creep in. Accurate pyrometers, which measure temperature with assurance and accuracy unattainable by the most expert eyes, were added to the mill equipment. Microscopes were set up in the roar of the rolling mills to check and control processes. As a result fewer shipments failed to meet the needs of the purchasers, rejections were reduced, and more money flowed into the pocketbooks of the shareholders.

In establishing the laboratory control of copper and copper alloy manufacture it was necessary to map the new field of science that was explored. All the mixtures of copper and zinc, the alloys known as brass, were studied at various temperatures and

their physical state and properties were charted on diagrams. The size of grain, hardness, tensile strength, and stretching quality of each alloy, treated in various ways, was carefully determined and recorded for the guidance of the mill metallurgists and chemists and for the archives of science. Upon these maps, in which composition is plotted against temperature, the lands of "alpha" and "beta" brass were outlined. Metallurgists and factory workers coöperating learned that alpha, or low-zinc, brasses can be drawn cold, while the "beta," or higher-zinc content, brasses must be heated in order to work. The research went on, dictated in part by the need for knowledge in the mill and in part by the scientific curiosity of the investigators. Bronzes, the mixtures of copper and tin, were likewise charted, and alloys of three and four components were also studied. Maps of copper-nickel-zinc alloys and aluminum bronzes were made. From the beginnings in Bassett's laboratories, the investigations in this new field spread to universities where students with fresh eagerness carried on the explorations.

At the center of all this probing into metal and its properties lies the great problem of the constitution of matter.

Watson Davis in *The Story of Copper* presents a graphic description of the atomic world of metals. The "inside story" of copper as he tells it is as penetrating as an X-ray, as revealing as the research

itself, and a classic example of scientific reporting. "Probably the most curious phenomenon of all nature is the way almost all the atoms in the world insist upon arranging themselves in orderly positions. Whenever they find themselves free to move, as when they make up a gas or are in a liquid, either a solution or a molten substance, the atoms rush out with a good deal of energy, but as soon as they, from cooling or other cause, find their range of activity decreasing, they face about and get in line with other atoms, the lines form squads, the squads platoons, and soon a crystal is built up. It is possible, by controlling conditions very carefully, to build up the whole number of atoms into one very large crystal, but ordinarily there is too much hurry and confusion for this to happen. Crystallization starts usually in many places throughout the liquid at the same time. The near-by atoms hasten to join with those whose ranks are closing up, and the crystals grow. But before they have grown very large they begin to interfere with each other. They actually push against each other, and many different things may happen. One striking result of the struggle is twinning, when two crystals headed in opposite directions grow through each other. Twinned crystals are characteristic of the copper alloys. Often the crystals are squeezed into other forms than the one they are trying to assume, and queer, misshapen forms occur.

"Copper, like most metals, tends to arrange itself in cubes, or in double pyramids. As crystals are described, these forms are the same; each figure has three equal axes of symmetry at right angles to each other, running through the figure and connecting the opposite faces, or points, as the case may be.

"Metallurgists are vitally concerned in the fundamental connection between crystalline form and the hardness of the metal. They know that working a metal makes it harder and that it also deforms the crystals, drawing them out in the direction of working and squeezing and flattening them in the other direction. But the resistance of the crystals to this treatment and their tendency to return to their normal shape make the worked metal brittle as well. Therefore most metals, after working, must be heated to some tempertaure below their melting-points and held there for a while until the crystals have recovered from their strenuous experience. This is annealing. It restores the metal's malleability or ductility, but, unfortunately, softens it up again. A nice balance must be worked out between the two extremes.

"With his recently developed sixth sense man has been able to probe into the very depths of metals and learn their structure. The X-rays, shorter and more powerful than the light-rays that we see, have made known the arrangement of atoms in the metallic crystals with the same clarity with which they

show the framework of the human body. When Laue found in 1912 that X-rays of uniform frequency or wave-length projected into a mass of fine crystals would be reflected in an orderly manner by regularly spaced atoms of crystals, he gave the metallurgist a new tool and he opened what had been a closed door into the interior of metals.

"The X-ray shows that atoms of copper form in the same shape as the copper crystals that are seen under the microscope. The copper crystal is a cube with an atom centered in the surface of each face.

"Having located the metallic atom, the metallurgist can find out more about it and its effects on visible metal, and he is proceeding to do so. At present it is said that only two X-ray spectrometers, one in England and one in America, are being used for metallographic research. These will undoubtedly multiply many times."

From the beginnings with one microscope, Bassett has seen his laboratory grow to a staff of an hundred men working with all the old and new apparatus for learning metallic secrets. And Bassett, while still in charge of the laboratory, now at Waterbury, Conn., is no longer just a chemist or metallurgist. He is an early member of that growing industrial order of technically trained executives. As one of a triumvirate he is in charge of the operation of all the American Brass Company's mills.

"The story of this research into the crystalline

structure of copper and copper alloys is quickly and easily told but it required a very large amount of patient and painstaking work and has consumed years of laboratory research," Bassett said in recalling his researches. "Practically none of the results were published until the knowledge of the work came into the hands of the university laboratories. Since then, publication has gone on rapidly and the facts have become known and appreciated and are the basis of the specifications for copper and copper alloys.

"The manufacture of wrought copper and its alloys is now carried out under pyrometric and laboratory control, and the microscope has become as much a necessary part of rolling mill equipment as the rolls themselves. The research made possible the manufacture of uniform metal of much greater perfection than was possible before. Such brass and copper are needed in the industries where the metals are used because their plants are arranged for mass production methods and imperfect materials slow down production and require tedious and expensive inspections."

While the solid groundwork of metallurgical data was being obtained from detailed studies in the laboratory, there were pressing problems to be met out in the great cities, harbors, and byways of the intensely practical world. One of these was the matter of condenser tubes, those necessary adjuncts to

great steam engines. Great liners were often in trouble due to tube failures, great power houses failed in times of need.

In coöperation with J. P. Sparrow, chief operating engineer of the New York Edison Company, an investigation was planned. The period of service of condenser tubes is important either in a power plant or on shipboard because leaky tubes mean a low vacuum and the introduction of condensing water into the boilers wherever, as in marine service, the condensed steam is used for boiler feed water, and the presence of sea water in boilers produces foaming and causes rapid corrosion.

Bassett knew that various classes of tubes had been tried and numerous varieties of failures had been observed. As in the case of surface etching for microscope work there was little exact knowledge as to the causes of failure or the corrodibility of metals and alloys under service conditions. The principal types of failure observed had been splitting, pitting, and dezincification. Bassett started by using the information he had already gathered. Short time tests were devised in the laboratory and samples of metals which had failed were studied with the microscope. Then such troubles as splitting and pitting were artificially produced in the laboratory. The time tests indicated that tubes lightly annealed did not split. In order to pack tubes into condenser heads and obtain a tight joint they should not be too

soft. This led to the necessity for determining the lightest annealing possible that would prevent splitting. Eventually, through lengthy experiments and several ingenious new methods, these faults in condenser tubes were eliminated. Knowledge of the crystalline structure was essential in the consummation of this work.

Even more difficult to solve was the problem of corrodibility. It had been found that Muntz Metal tubes, then most widely used, failed rapidly in sea water, the failure being due to dezincification, or the removal of the zinc from the alloy. Aluminum bronze, an alloy of 85 per cent copper and 15 per cent nickel, Admiralty Brass, and a number of other alloys were tried in the laboratory. In order to simulate actual conditions under which the improved tubes, the object of the research, would be used, the experimenters subjected them to tests with salt water taken from New York harbor. It was then learned that an Admiralty alloy containing 70 per cent copper, 29 per cent zinc and 1 per cent tin gave longer life and more consistently uniform service. Bassett and his associates then settled upon Admiralty alloy as being the best material at hand and they proceeded to watch its use in the great condensers of the various Edison plants. Experience indicated that sound tubes, free from casting defects which produced seams and slivers, gave the longest service. The investigation had to go on as rapidly as possible

because power plants were demanding larger and more efficient condensers. But "rapidity" is a relative term in the mind of the true research worker; haste is not permitted to interfere with sound work. In this case the experiments were continued for ten years!

"An arrangement was made whereby five tubes of copper and the same number from each of several copper alloys were placed between two wooden boxes eight feet apart," Bassett remembers. "The tubes were packed into the wooden headers, water being pumped into one of the boxes, flowing through the tubes into the second box and from there overflowing into a storage tank. The water used was taken from New York Harbor near one of the stations of the New York Edison Company and the test was carried on for ten years, the water being renewed monthly."

After about two years strips of the various metals were hung in the storage tank, the reason being that the difference in the tubes was likely to be so small that it would be difficult to determine which metal or alloy was the best by any available method of cleaning and weighing. The metal strips were taken out and weighed from time to time. And to further verify the results strips of the same composition were subjected to accelerated tests in salt spray. At the end of the ten-year period all the tubes and strips were dissected and their condition photographed. The research demonstrated that Admiralty alloy,

Low Brass so-called because the zinc content is only 20 per cent, and 5 per cent Aluminum Bronze were most resistant to corrosion from sea water. The result of the study was to more than double the life of condenser tubes without materially increasing the cost.

Such is only part of the story of what research has accomplished for brass and copper. With grains of copper, the elementary building blocks of the research worker, mortised with the practical experience of mill workers, another craft has found its place in the family of science-aided industries. Built on the solid foundation of scientific research, a structure has been builded and a memorial erected, which should be as enduring as copper itself.

CHAPTER THIRTEEN

POSITIVE NEGATIVES

C. E. Kenneth Mees

THE popular theory that research workers are professorial gentlemen wearing eyeglasses with thick lenses, stooped and round shouldered from bending over laboratory tables, absent-minded, a little bewildered upon emerging into the glare of sunlight—this popular fallacy has never been more clearly exploded than in the person of Dr. C. E. Kenneth Mees, Director of Research for the Eastman Kodak Company.

Mees is energetic, talkative, and, as becomes one who is by nature a pioneer, aggressive. His prominent Roman nose is bridged by spectacles, his hair is sandy—in his earlier years it may have been brown, and also less tidy. To-day, a habit of pulling at his forelock when speaking reveals that it is also sufficiently springy in nature to regain its position; an elasticity Time may destroy, leaving Mees with a frontal curl of Napoleonic type.

The keynote of Mees' success is a conscientious belief in himself—and his fellow beings. His ability is patent to all, while his idiosyncrasies are equally

C. E. K. Mees

public and form a source of private amusement to his friends. To those who enjoy his friendship, and they are many, his thorough boyishness is a constant treat. Mees will never regain his youth for the very good reason that he will never lose it. He does not suggest athleticism, but heaven aid the man who goes on hike with him and is not provided with stout and comfortable boots, for Mees will walk and talk as the miles are left behind. There are but few subjects which do not interest him; of the others he has the facility of treating them in such an attractive manner as to make a recital of solid fact appear as part of a romance.

And if, during the journey, Mees has incidentally remarked upon the peculiar absorption (color to Mees is always a matter absorption) of a wayside flower, now he will be interested in speculating as to the peculiar anitine product which has been in coloring the jelly or any other food offered suggesting the adventitious, although remote, work of the chemist. The conversation will not flag. Mees has three generations of Nonconformist ministers behind him, and to him talking is seldom tiring. Added to this he possesses a very retentive memory and a passion for reading. He is catholic in his choice of books, the most erudite treatise and the lightest fiction are in his library.

Science to Mees is, in a manner of speaking, his religion, and he has the fervor of the fanaticist in

his earnest search for truth and knowledge. He has no use whatever for charlatans, and woe betide the individual with spurious facts who tries to put them over. On such occasions, Mees uses facts as a soldier uses hand grenades, and the result is as devastating.

His power of explaining intricate problems in a simple form is an ability which may be properly termed a gift. Indeed, there are among his friends those who feel that the scholastic world lost a great teacher when Mees devoted himself to research.

There is no conservation in Mees' nature. That which has been is not, to him, necessarily that which should best continue. He is intense in his belief (and practice) that all new knowledge should be utilized and is never more enthusiastic than when confirming its strength or proving its weakness.

"The science of photography," Mees says, "is the chemistry and physics of light sensitive just as physiology is the chemistry and physics of living organisms."

The research laboratory at Kodak Park, like those at the General Electric Company or the General Motors Company, are based upon the modern industrial axiom that progress comes through the harmonious working of groups of men. The day of the individual genius has passed, and Mees has unquestioned talent for keeping his subordinates stimulated, for encouraging them when their experiments seem to be leading nowhere, for directing and coördinating

their activities. His intolerance of laziness and foggy thinking may cause him to seem rather brusque to the outsider. But to the men who are under him he is sympathetic, tolerant of mistakes, appreciative of good work. His associates did most of the experimenting which resulted in the perfection of the film used in home motion pictures. The process whereby such films were made and, later, developed is as pretty a technical one as can be found in any industrial research laboratory in the country. Established methods were cast aside. The negatives were so treated that they became positive and the originals used in projection machines, at enormous saving to the home movie enthusiast.

L. H. Baekeland, discoverer of the synthetic resinoid called Bakelite and the inventor of Velox photographic paper, was attracted to the field of photography because so many of its apparently simple chemical reactions were complete mysteries to science. Mees went into the work for much the same reason. The son of a Wesleyan minister, he was born at Wellingborough, England, in 1882 and early decided upon a technical career. He was not, however, certain regarding the particular subject he would select. After attending St. Dunstan's College at Catford, a school specializing in scientific education for boys, he went to the University of London. There, between 1900 and 1906, he worked in the laboratory of Sir William Ramsay and did

his first research work. A brother had been an enthusiastic amateur photographer, and Mees also had become interested in the subject. While engaged in research work in chemistry it occurred to him that little or nothing was known of the underlying science and he received permission to make this the subject of his thesis. In 1903 he was awarded the degree of B.Sc. for a study dealing with photographic sensitometry. He next collaborated with a fellow student, S. E. Sheppard, who is now his first assistant at Kodak Park, on the photographic theory. Each received the degree of D.Sc. for this work, which was published and which is still an authoritative work even though more than twenty years have passed.

Mees no longer had any doubt as to his career. As soon as he left London University he entered the firm of Wratten & Wainright, Ltd., manufacturers of photographic plates with a small plant at Croydon. Wratten & Wainright enjoyed a high reputation for the quality of their products and every facility for research was given to Mees. It was only a few months after he had joined the house that he was able to place on the market a "panchromatic" plate, sensitive to the entire visible spectrum, the forerunner of modern plates sensitive even to infrared, which is invisible to man. The following year many colored filters, further improving the color sensitivity of photographic plates, were developed,

and constantly there came from Mees' laboratory booklets and pamphlets describing his experiments. In 1907 "Wratten Safelights," consisting of gelatin-coated glass were introduced for the illumination of dark rooms. Mees was learning, obviously, a good deal about the photographic science. But he was becoming further convinced, also, that the surface had only been scratched. Like Baekeland, whose first interest was photography, he was constantly annoyed by the fact that no one knew what happened to silver emulsions when struck by the light. Obviously something occurred, because after exposure the silver could be separated by a developer and a picture produced. Mees has spent many hours working on this, but he is still puzzled by the mysterious reaction.

The scientific articles which he wrote while with Wratten & Wainright established Mees as a leader in his field. In 1907, when only twenty-five years old, he was given the Henderson award for scientific research in photography. The following year a paper on color photography brought him the silver medal of the Royal Society of Arts. In 1913 he was awarded the Progress Medal of the Royal Photographic Society. Meanwhile, years before, a young American bank clerk in Rochester named George Eastman had become interested in photography. Those were the days of the so-called "wet" plate. The prehistoric amateur photographer had to carry

with him a cartload of apparatus, including a dark tent in which to sensitize his plates. Eastman, although untrained, had the spirit of the industrial explorer. Looking forward to a holiday in the West Indies, he thought that it would be enjoyable to take some pictures on his trip so he engaged a photographer to teach him the little that was then known about photography.

Somehow it occurred to Eastman that he might make photography easier for the amateur and his resolve was strengthened when he read about a new plate which had been made. This did not have to be developed while still wet. He experimented in manufacturing these plates and he was soon selling so many of them that he resigned from the bank and built a factory. This was in the early eighties. Dry plates made unnecessary the dark tent, the nitrate bath, and other cumbersome details. But they were still bulky and it was in an effort to provide a lighter medium that Eastman perfected the photographic film. In 1888 he invented the box camera to be used with a roll film and this he named, after seeking a selection of vowels and consonants that would be easily remembered, the "Kodak." Untold wealth was just around the corner. By 1891 the first building was erected on the outskirts of Rochester on a plot of land which he called "Kodak Park." Now the plant covers four hundred acres. It has its own streets, sewers, water system, railroad

tracks, power plants, and fire department. Its products are shipped to all parts of the world. So complete is its growth that to-day the site is a "park" in name only. One of the last remaining stretches of lawn and trees must soon be torn up for a new research laboratory that is to be built.

Eastman had long employed scientists in the various departments of his great plants, but he felt that no well-organized work was being done in the United States to establish photography as a science. The name of Mees was already well known to him and when the Englishman came to America in 1909 he was asked to visit Kodak Park. He had crossed the ocean to confer with the American Bank Note Company on the color printing of securities, but returned to Wratten & Wainright, the British photography house, after his visit with Eastman. The Rochester manufacturer continued to meditate on the advisability of inaugurating a research laboratory and in 1912 he purchased the business of Wratten & Wainright. A vital part of this business, obviously, was the skill and knowledge of C. E. K. Mees, its technical expert. And although he was, naturally, not for sale along with the other assets he consented to come to Rochester to organize a research laboratory.

"The early years were occupied in laying the foundation on which the future could be built," he recalls. "There were great gaps in the theory of

photography. The subject had been developed very largely by amateurs who had worked only on the parts of it in which they were interested, and it was necessary to study all sides of the subject and to build a consistent structure of the science of photography on which future progress could be based.

"In most of the applied sciences the fundamental work is carried on very largely in the universities. Behind the chemical industry stands the professor of chemistry; behind the automobile industry, the mechanical engineer; behind the electric industry, the physicist; but photography is not studied in the universities as a science, and the science of photography is dependent at the present time on the research laboratories of the manufacturers for its progress.

"After the theory, as the work of the laboratory got under way, came the application. Step by step, the research laboratory of the Eastman Kodak Company began to apply its science to the products of the company. Its work has been keenly felt in photography in commerce and industry, in the professional movies, in aerial photography and in the many other fields where photography is of significance."

From the research laboratory over which Mees presides have come, also, tools for the metallurgist, instruments for the physician and for the astronomer. Not long ago, to cite an example of its work, a botanist called on the X-ray experts of the General

Electric laboratories at Schenectady. He had been able, this scientist said, to make motion pictures of seed germination. He needed a small X-ray machine and a motion picture camera, specially adapted to his purpose, to carry on the work. It would greatly assist him, he said, to use a color-sensitive film.

"We'll supply the X-ray," said the General Electric man. "Go to see Mees at Rochester about the rest of it. If any one can help you, he can."

The commercial motion picture was the fruit of the combined inventive genius of Thomas A. Edison and George Eastman. Edison made the apparatus, but could not use it without a film. Eastman made the film. But practical, reasonably priced motion pictures in the home are the result of long, detailed, painstaking work in which many of the 180 men and women in the Eastman research laboratory took part. It was soon after the end of the war that the staff of the research laboratory decided to develop amateur motion pictures. The professional motion picture camera was too heavy and too expensive. It had to be used with a tripod. The cost of the film and its developing and printing, including a negative to be discarded by the average amateur after one or two prints had been made, was prohibitive. Smaller cameras and cheaper film must be produced.

During the years that had passed since the inauguration of research work at Rochester, Mees had gathered quantities of data applicable to this prob-

lem. The obvious way of producing cheaper film was to make the films smaller. This had been attempted, in the past, and it had been found that a miniature positive produced from a small negative showed, when magnified many times for the screen, a grainy structure. This was as obvious in the home as it would have been at the theater, for the home audience sits very close to the screen.

But of even greater importance was the cost of printing the positive. In theatrical motion pictures dozens of positives are made from the one negative, and the cost is apportioned; the amateur desired only one or two. Besides, amateurs are always in a tremendous hurry to see the results of their artistic efforts and to make prints would double the period between taking and delivery of the finished film. The solution lay in changing—by means of the wand of research—the original negative into a positive and shipping it back to the customer.

Previous studies had demonstrated that this was feasible, at least from a laboratory point of view. The results had been far from satisfactory, however. So John G. Capstaff worked out a new process. The films, after development, are bleached and then reexposed. The metallic silver deposited by the developing fluid is removed by the bleaching solution. Then the film is again exposed to light and again developed. The result is a positive. To-day it is all done by extraordinary machines, perfected

by research workers, which seem almost to have the power of thought. When the amateur photographer has under or over-exposed his film, the machine corrects his error.

The process can be better understood, perhaps, if the chemical reaction is stated. The new motion picture film, like every other form of film or plate, is coated with a silver combination held in gelatin. Light so affects silver halide (the combination used in this case) that upon treatment with a developer it turns into black, metallic silver. The brightest parts of the object being photographed, therefore, turn the corresponding areas of the film black, and a negative results. The process known as "fixing," with which every amateur is familiar, is simply a method for removing the unexposed portions of the silver halide. This corresponds to the darker parts of the object being photographed and has not been affected by the light.

The first change in the new process comes after the film has been developed. Then, instead of being "fixed," it is run through a bleaching bath which removes the metallic silver but leaves the unexposed silver halide untouched. The film is then rinsed and is exposed to a white light of varying brilliance. And here, at least to the layman, is the most astonishing step of the entire treatment. The machines now used for the purpose carry the film on reels which pass first through the developer and then through

the bleaching bath. Then they pass under a light which is controlled. If the film is too light (has been over-exposed by the amateur), it is given additional brilliance during this second exposure. If it is too dark (has been under-exposed), light shines less brilliantly.

Each of the tiny images on the film has now been developed, bleached, and exposed again. The parts which were negative have become positive and again the film is developed, run through a fixing bath, and automatically dried. The film is then run through a projector to make certain that the machines are operating properly. The inspection work goes on adjacent to the developing rooms and, although it may be dull stuff to those in charge, it is fascinating for the stranger to watch. Amateur motion picture enthusiasts, it seems, pick out the most extraordinary things to photograph! Most of the films, it is true, are travel scenes and some of them are remarkably good. But others are interior views of rooms, with not a soul moving in them. Under its present system the Eastman Kodak Company develops all motion picture films without charge; the cost is included in the price of the film itself.

Many other problems had to be solved before the Cine-Kodak and its companion projector could be placed on the market in their present form. These, though, were largely mechanical and engineering details and were not difficult. Small cameras had to

be developed to handle the smaller film, which is $\frac{5}{8}$ of an inch wide. Cameras driven by spring motors and projectors run by electricity were soon perfected. Within five years the mere idea of amateur motion picture photography became an actuality and tens of thousands of families have now purchased the equipment. Departments have been established for making duplicates of amateur films, for making reductions from standard motion pictures. Professional motion pictures may now be rented for use in the home, just as circulating libraries rent books.

Even more important, from the public point of view, has been the educational feature. Eastman early realized that the small, noninflammable film could be used in schools and he set aside a sum of money for experimental purposes. Dr. Thomas E. Finnegan, for many years connected with the New York State Department of Education, was employed to conduct the research and during a period of two years some thirty films were made and tested on 6000 boys and girls in twelve cities. Last May the work reached a stage where commercial development of the idea was possible and papers of incorporation have been filed for Eastman Teaching Films, Inc., a subsidiary of the Eastman Kodak Company. The capital stock is $1,000,000. Dr. Finnegan is the president, and Mees is vice president and a director of the new company. Many educational films will be made within a year.

The primary purpose of his laboratory, Mees insists, is to learn all there is to know about photography and he is the first to admit that much of the science is still clouded in mystery. As much time as possible is devoted to pure research. The rest is divided between what Mees calls fundamental development (problems similar to the reversible film process), immediate development (the perfection of the process and the designing of machines for commercial use), and plant service. When things go wrong in one of the many plants of the company, the research laboratory is frequently called upon to locate the trouble, prescribe a cure, and see that the cure is properly administered.

Last April, Mees had 183 men and women in his department. New men are constantly being taken on, usually chemists and physicists from the universities and colleges, and these are encouraged to work out such problems as may interest them, as long as they are related to photography. Mees is, naturally, in intimate touch with everything that goes on and he believes that the men he secures are adequately trained. But once in awhile he sighs for the old-time inventor.

"I have not," he explains, "the slightest trace of the inventor in my make-up. And the inventor is a valuable man in any research organization. He may be hard to get along with. He may even be ignorant, because it is difficult for the inventive mind

to absorb facts. But he's essential. He finds a path around or across when the research man has been stopped. He may fool around for years without getting anywhere. But he's essential. When you get your hands on an inventor freeze onto him!"

CHAPTER FOURTEEN

IT WON'T BREAK

E. C. Sullivan

A CHANCE visitor to the research laboratory of the Corning Glass Works at Corning, N. Y., some fifteen years ago must have been vastly puzzled. Pleasant aromas of bread and cake were likely to fill the air. Occasionally these were replaced by the fragrance of a pudding reaching perfection. But the most mystifying feature of the laboratory was over in one corner where three men, scholarly in appearance and absorbed in what they were doing, were bending over a table. From the corner came the sound of weights being dropped on chinaware and, not infrequently, a grunt of satisfaction from one of the men.

Closer examination revealed that Dr. E. C. Sullivan, research director of the company, Dr. J. T. Littleton, Jr., chief physicist, and W. C. Taylor, chief chemist, were conducting experiments to test the resistance of a new glassware, later to be known throughout the world as Pyrex. They had on the table inverted dishes and bowls of chinaware, enameled earthenware, and the new glassware and

were dropping, from various heights, a small weight on top of each variety. Some of the earthenware dishes, they found to their glee, cracked when the weight was dropped from a height of about six inches. Others cracked at eight inches and shattered at eighteen. The crockery broke at sixteen inches, when it had a fairly thick bottom, and at four inches when the bottom was thin. But the glassware withstood the weight falling from twenty-two inches before it cracked and was not shattered even at thirty-four inches.

Some time afterward the United States Bureau of Standards conducted similar tests and an official report, entitled "Comparative Tests of Chemical Glassware" further established the reputation of Pyrex ware. The new product, these experts stated, was "far superior to any of the other wares" in its resistance to shock. Translated into everyday terms, this means that the housewife using Pyrex ovenware with reasonable care is not likely to break it. Nursing bottles fashioned from the glasses sold under the Pyrex trade-mark can be heated at night without fear of the domestic tragedy caused when nursing bottles break and there is no more milk in the ice-box. Meanwhile the baking and other cooking tests at the Corning laboratory had demonstrated that Pyrex ovenware had many superior qualities, one of them being that it bakes food more rapidly and more thoroughly than older wares. It is clean and is not

affected by the chemicals to be found in foodstuffs.

So well known is Pyrex ovenware in the household that few people realize that heat-resistant glass has many other uses. Its high resistance has made Pyrex Radio Glass invaluable as an insulator and was used on the plane of Commander Richard Byrd on his historic flight across the North Pole. Pyrex glasses are equally valuable in the high-tension insulator field and interruptions to power service caused by thunderstorms are reduced by their use. Railroad men swing lanterns fashioned from Pyrex glasses—one of the first demands for a nonbreakable glass came from the railroads—and now trains will no longer be wrecked because a lantern fell to pieces on a cold night.

Dr. George Ellery Hale, Director of Mount Wilson Observatory writing on "The Possibilities of Large Telescope," in *Harpers* has paid tribute to research in the glass industry with this statement: "Recently, important advances have been made in the art of glass manufacture, and mirror discs much larger and better than the 100-inch can now undoubtedly be cast." Pyrex glasses, so useful in the kitchen and the chemical laboratory because they are not easily cracked by heat, are also advantageous for telescope mirrors. "Observations must always be made through the widely opened shutter of the dome, at temperatures as nearly as possible the same as that of the outer air. As the temperature rises or falls the

mirror must respond." The small expansion or contraction of Pyrex glasses means that mirrors made of them "undergo less change of figure and, therefore, give more sharply defined star images—a vitally important matter in all classes of work, especially in the study of the extremely faint stars in the spiral nebulæ."

The heating and cooling of acids and acid solutions were difficult when other wares were used, but to-day chemical plants have literally miles of Pyrex tubing and scores of tanks and retorts. The comparative thickness of Pyrex ware reduces breakage in the laboratory. Its hardness makes it adaptable for rollers in textile mills. Radio experimenters, attempting to devise "directional beams" which will give privacy to aerial communication, use Pyrex insulators. The glasses developed by the Corning Glass Works, like the metals of the Crucible Steel Company, are special glasses resistant to heat, corrosion, breakage. The qualities of the new glasses are the fruit of long hours of research covering years of labor. Seven years, for instance, were needed to produce the glasses sold under the Pyrex trade-mark and work is still going on.

It is an interesting coincidence that Sullivan, who has directed the development of a ware which competes with the sale of aluminum cooking utensils, should have been inspired to take up chemistry by a

high school teacher's prediction that a fortune awaited the man who could devise a commercial method for extracting aluminum from clay. Born in Elgin, Ill., in 1872, Sullivan attended the public schools of Chicago and was first exposed to chemistry in high school. It was there that an instructor painted alluring pictures of the untold wealth in industrial chemistry. Sullivan resolved to annex some of this wealth and entered the University of Michigan, at Ann Arbor, to learn everything that he could about chemistry. He was graduated in 1894 and went to work in a dynamite factory in Indiana. Having survived this, he transferred to a baking powder plant and there met a German chemist who told him that, after all, he knew very little chemistry. The thing to do, the German said, was to attend Göttingen and Leipzig where he could work with renowned scientists, Nernst and Ostwald. Opportunities for studying chemistry in America then were not what they are to-day, and Sullivan followed this advice, after saving money enough to live on while in Germany, and in 1899 he was awarded his Ph.D. at Leipzig. He returned to the United States to seek a job and for four years taught analytical chemistry at the University of Michigan. He still hoped, however, to get into industrial work and left Michigan to engage in research in silicate chemistry at the laboratory of the United States Geological Survey. This work prepared him for the post he

took in 1908, chief chemist and laboratory head of the Corning Glass Works. In 1920 he was made a vice president and a year later was placed in charge of all the manufacturing activities, including not only the production of Pyrex but the manufacture of millions of electric light bulbs annually, of vast stores of glass tubing, and of artistic and architectural glassware.

Sullivan finds glass, a material viewed by most people as exceedingly simple, filled with mystery and fascination. Look into the depths of it long enough, he has said, and "one falls easily into the mood of the crystal-gazer who, looking into the interior of the polished ball, observes a mist and then a blackening, followed by the emergence of pictures which seem to develop from the play of light and color and shadow." The purpose of research in the glass industry, he believes, is to make the material less breakable by heat, to increase its resistance to corrosion and so to control the transparency that colors can be produced with accuracy. In addition, study is being made of the properties of glass as an insulator, the perfection of new machinery, and production in large quantities. Glass manufacture is no longer a craft handed down from father to son. Most of the work once done by the traditional glassblower is now accomplished automatically. The new glasses, among them Pyrex, are the development of patient research, research in which there were frequent near-disasters

and disappointments. Let us hear Sullivan telling the story in his own way:

"One super-resistant glass originated in an effort to improve the railroad lantern globe. A railroad trainman's lantern may happen to rest in a tilted position so that the flame plays directly on the globe, overheating the glass in a single spot. Taken out into rain or snow such a globe of ordinary glass is likely to crack, and failure of a signal lantern at a critical time may easily lead to disaster. It was in the development of a material to meet such severe service that the glasses originated which have become known under the Pyrex trademark.

"The physical property of glass which it is most convenient to change in order to make the glass stand rapid heating or cooling is the thermal expansion. When a hot glass object is chilled the part cooling first is likely, as it shrinks, to tear away from the hotter portion. In a glass of low coefficient of expansion the hot and cold portions are more nearly of the same volume, and the tendency to rupture is therefore reduced.

"Otto Schott of Jena had been the pioneer in the development of low-expansion glasses and had put on the market the well-known thermometer and laboratory glasses. Our problem was to carry the expansion to a still lower point and at the same time to improve resistance to the solvent action of water

and chemicals and maintain satisfactory qualities of melting and working.

"Although lantern globe glass was proven entirely satisfactory for lanterns and other ordinary purposes, it lacked the very high resistance to attack by water and acid and alkali requisite for a container for baking food. Thousands of systematic experimental melts, the physical and chemical properties of the glass in each melt being carefully followed, led, through a period of seven years, sometimes by way of detours and again with retracing of steps, to the ultimate results.

"With millions of glass baking dishes in use for the past ten years, it is a little difficult to bring back clearly to mind what seemed then the wildness of the idea that glass dishes could be sold commercially and used successfully for household baking. Glass was fragile. It was terrifying to think of the consequences of splinters of glass getting into the food. Improvised dishes were, however, put into service by a few venturesome housewives, and later, faith and courage growing slightly, a pie-plate mold was made. The pies baked in glass were acceptable, and to gain wider experience and at the same time help a good cause, it was proposed to allow a local church bazaar to sell a few pie-plates at whatever price might be obtainable. But discretion prevailed and the suggestion was finally rejected on the ground that we didn't want to get a black eye in our own home town by

sponsoring the use of glass dishes for baking in an oven.

"In a later moment of faith Colonel Blank, who had taken an enthusiastic friendly interest in the matter, had a pie baked and served in the glass plate at a well-known New York City hotel which had been his residence for years. As he sat at dinner sharing the pie with friends and convincing them and himself of the merits of the new utensil, he became aware of a sharp foreign substance in his mouth which, furtively removed and examined, proved to be a splinter of glass. The glass dish development might have died at that moment if the Colonel had been given to hasty conclusions. Actually he inspected the pie-plate at the first opportunity and found that it had lost no splinter, and further investigation revealed that the fragment was a bit of ordinary glass, probably from a vault light under which materials were stored. By such threads do our enterprises hang!

"Then the country's foremost cooking expert was consulted. Would she make tests of the glass dishes? Her reply aroused gloomy forebodings of the attitude the public might take toward glass to be used in kitchen stoves. Her experience with glass had been such that the idea of putting glass dishes into an oven did not appeal to her. She would try the ware but would report things only as she found them. An unfavorable opinion from this source undoubtedly would have ended the entire matter. After a period

of anxious anticipation an enthusiastic letter came. The glass dish had been used for baking an ice-cream pudding, a severe test, and had proved more than satisfactory.

"Up to this point the verdict was, on the whole, favorable. But the tests had been entrusted to skilled hands, more or less directly interested in a successful outcome. A research worker or a laboratory man's wife who will follow instructions can with perfect safety bring water to boil in a glass teapot, but the average housewife is apt to come to grief in such an operation. What treatment would glass dishes receive in a kitchen, and how long would they last? That was the burning—or rather breaking—question.

"Trials over extended periods eventually dissolved all doubts, and the obstacles which had appeared so menacing dwindled and vanished.

"An unlooked-for outgrowth of the tests was the discovery that the baking process was shorter in glass than in metal. This was contrary to the generally accepted belief that the thick wall of glass would cause the food to heat more slowly than in the usual thin metal container. The fact was confirmed by various methods and the reason for it was then recognized in the high reflecting power of metal and the low reflecting power of glass for heat rays.

"The relative influence of glass and metal on baking was strikingly shown by an experiment devised

by Dr. J. T. Littleton. Opposite quarters of the bottom surface of a glass baking dish were silvered and a cake was baked in it in the ordinary manner. Where the cake had been protected by the metal coating the bottom was light-colored, sticky, and imperfectly baked, while in the other quarters it was brown and well done. As the cake was turned out of the dish it adhered to the glass over the silvered sections, and the quartering was plainly visible on the bottom of the cake.

"The properties which fit a glass for service in the kitchen stove are those which fit it for laboratory use—resistance to the dissolving action of chemicals and power to stand up under rapid and uneven heating. Baking-ware of glass was introduced to the market not long after the breaking out of the Great War, which stopped shipment of laboratory glass to this country from Germany.

"One day there entered our laboratory a professor from one of the largest universities on the Atlantic seaboard. It was his responsibility to arrange for laboratory supplies for his institution, and he had brought with him a custard cup purchased from the first New York City store to handle glass baking-ware. The professor had made some tests on the glass dish and wanted to know if his university could not be supplied with beakers and flasks of the same glass. Beakers had already been made in a small way but for various reasons had not found much favor. It

appeared then that the volume of business would not be large and a considerable investment in molds would be called for because of the variety of sizes and shapes of ware required by laboratory workers. But the demand became insistent, even the United States Government adding its importunities, and manufacture was begun.

"One day, there was a demonstration in the works laboratory for the benefit of the head of a supply house, for the purpose of persuading him to take on the new ware of baking-dish origin. During the demonstration one of the beakers rolled from the laboratory table and fell on the hardwood floor. As luck would have it the beaker bounced around and finally settled down unbroken. This incident carried more weight than all the carefully worked out measurements, and the day was won.

"The new ware found favor with chemists for three reasons. First, because of its resistance to the dissolving action of liquids in contact with it. It is most important to the chemist and physicist that the containers in which their operations are carried out cause as little contamination as possible to the substances under investigation. No container known—not even of platinum—can be depended upon in chemical and physical work not to contaminate its contents. Therefore, the container is chosen that will cause a minimum of harm, either from the kind or the quantity of material dissolved from it. Con-

tamination by glass always was a factor in very accurate analyses. When a glass which reduced such contamination to one-fourth or one-fifth was developed it was adopted at once.

"A second reason for ready acceptance on the part of laboratory workers was resistance to breakage by heat. Time-consuming precautions in heating were to a considerable extent eliminated.

"A third outstanding quality in the new laboratory ware was its resistance to breakage in ordinary handling. Ordinarily thick glass is more likely to break than thin when quickly cooled or heated and for this reason earlier beakers and flasks had been made of almost eggshell thinness. The new ware, on account of its low expansion coefficient, could be made thick-walled without sacrificing thermal endurance. Simply placing a full beaker or flask on a hard-surface bench is a more or less hazardous operation when the glass is very thin, and the more robust ware has made possible a freer use of the stone-top laboratory table by those who prefer the stone to the wooden top.

"So cordial was the recognition of the merits of the new American ware that foreign laboratory glass has never regained its foothold in this country and the brand of home manufacture is used almost exclusively.

"For present-day bottle-fed babies, feedings for 24 hours are prepared and put on ice, each feeding

in its own bottle, to be warmed in hot water for baby's consumption as the successive feeding times come. A bottle of ordinary glass sometimes breaks in the sterilizing process, but worse yet it sometimes breaks in the process of reheating the food—sometimes in the small hours of a winter morning when it contains the only remaining prepared feeding. The glass developed for grown-ups' baking dishes has come to the rescue of baby and parents and is used in large quantities for nursing-bottles with comparative security against breakage caused by heat.

"In France as well as in the United States the nursing bottle has been particularly successful, and this mention of France reminds us to set down the fact that the glasses manufactured in America under the Pyrex trade-mark are now produced abroad not only in France but also in England and Germany and are shipped to most of the world."

Such is the story behind Pyrex. Obviously Sullivan is a man well able to convince the directors of his company of the value of research, for he is able to speak in terms that are comprehensible to the layman. On his staff at Corning he has about fifteen scientists and he is convinced that the most difficult of all the problems facing the research director is the smooth, efficient, coördinated operation of the laboratory.

"We have to learn," he admits, "to work together as a team and with others. We scientists are of a temperamental type and need stimulation, encouragement, appreciation."

CHAPTER FIFTEEN

ATOMS IN CONCRETE FORM

Franklin R. McMillan

THE gigantic railroad viaduct, which carries swift limited trains and thundering freights across rivers and valleys, stands firm and unshaken because men made themselves familiar with the atomic structure of cement and concrete. Familiarity, in science, breeds not contempt but respect. When men knew the fundamentals concerning lime, alumina, and silica in the form of cement—which combine with water to make concrete—they knew what strains could be withstood by a concrete bridge. When they had found out how concrete became hard, what happened to the water used in mixing, what happened to the inner structure of the compound—when they had learned these things they dared build on bolder scales. Theories evolved in laboratories developed into facts. The scientist worked with the skilled craftsman and the engineer to perfect a material once as little touched by accurate knowledge as the mud-pies of a child of three.

The man to-day in command of those who study

A. R. McMillan

concrete is Franklin R. McMillan, Director of Research for the Portland Cement Association. A large, jovial, hearty individual, McMillan is glad to have visitors at the association's laboratories in Chicago. He welcomes the opportunity to assure his guests, particularly those to whom concrete is, vaguely, a kind of clay or mortar, that a real science has been developed. He hurries them down to humid testing rooms in the cellar or places a pillar of concrete in a giant press and shows them that it will withstand 100,000 pounds. The secret of successful concrete construction work, McMillan insists, lies in the engineer's adherence to instructions. But before that, the man who made the cement must have been equally willing to concede that the technical man in the laboratory is a true guide to fundamental principles, basic facts, and practical application.

Like most of the men destined to become directors of research, McMillan was early possessed by an insatiable curiosity regarding the things around him. He was born in 1882 at Worthington, Minn., and during his preparatory school days he had decided to become either a chemist, a physicist or an engineer. Unlike such men as Whitney of the General Electric, however, he was little interested in knowledge for its own sake. Having accumulated facts "A," "B," and "C," he was anxious to apply those facts to a definite problem. And it was this trait, no doubt, which caused him to become a civil engineer. It is,

at the present time, the dominating philosophy of the laboratories which he directs. The Portland Cement Association is not interested in pure science except to the extent that this relates to a very definite project—the improvement of cement and concrete.

Having been graduated a civil engineer from the University of Minnesota in 1905, McMillan engaged in railroad and construction engineering for five years. He built bridges, tunnels, roadbeds, and irrigation systems for the United States Reclamation Service. Then, feeling that he needed thorough training in fundamentals, he returned to the University of Minnesota for graduate work. He supported himself as an instructor, at first, and then became an assistant professor of structural engineering. He had worked with concrete in buildings and in structures for the reclamation service, but he now had an opportunity to make a detailed study of that complex substance. He remained at Minnesota until the United States entered the World War. Then came the cry of ships, more ships to carry an army to France and some one had the idea that vessels could be fashioned of concrete. McMillan had an important part in the work of the Concrete Ship Section of the United States Shipping Board, which built a number of concrete ships. In connection with this work he invented the "stainagraph," a gauge which records stresses in both steel and concrete ships at

the times of launching, dry docking, and at sea. But concrete ships did not prove practicable.

"We built some," McMillan says, "but they were very expensive. They would float well enough and carry adequate cargoes. But they could not compete with the conventional construction of peacetime operations. We learned, however, many valuable things about concrete during that period."

The war over, McMillan no longer had any doubt that cement and concrete were to be his specialties. After leaving the Shipping Board in 1920, he practiced as consulting engineer in Minneapolis and advised other engineers regarding concrete and cement problems. In 1924 he joined the staff of the Portland Cement Association.

The purposes of the association are varied—many of them unrelated to science. A large staff prepare and distribute literature designed to increase as well as improve the use of cement and concrete. Farmers, who use amazingly large quantities of both are circularized. Engineers are given data designed to encourage the use of more concrete. Contractors are told of its qualities. New uses for concrete are evolved and provide basic material for commercial exploitation through newspapers and magazines. Articles, written for popular or general appeal as well as for scientific readers, are sent to magazines describing new developments in the industry.

In 1915 the directors of the association felt the need for detail study into the properties of the materials which were being made in such vast quantities by the cement manufacturers of the United States. Lewis Institute, a polytechnic school in Chicago, agreed to coöperate with the association, and a small laboratory was started. "Learn all you can about concrete," was the charter under which the work was started. It was not an easy assignment, for men had been making cement for decades. They felt, not without justice, that they knew a good deal about their subject. Occasionally they resented the interference of these test-tube experts.

In time, however, the laboratory was able to define its problem. There were two outstanding aspects, the scientists found. The first was the study of cement itself. The second was the establishment of laboratory and field methods for testing products made with cement. The member companies of the Portland Cement Association had already imposed on themselves abitrary standards for their product; minimum quality standards which were really very high. But no agreement had been reached as to testing and much of the first work of the laboratory was along this line. Gradually the new standards were formulated; including those for testing, for preparing samples for testing, and even standards for the water used.

To-day the research work of the association is

carried on both in Chicago and in conjunction with the Bureau of Standards at Washington. Most of the really scientific work is done at the Bureau of Standards, but the Chicago laboratories are doing important work in testing the finished product. Chicago might be called the "trouble center." What's the matter with this sample, some members will ask? Tests are made and an answer given. Other tests determine the cause of construction difficulties. McMillan and his assistants are constantly in touch with construction engineers advising, counseling, and occasionally admonishing them.

Scientific methods require, of course, the apparatus of science, and the laboratories in Chicago have been supplied with everything that could possibly be needed. Three of the six floors in the association's building are devoted to the laboratory. The roof is used for weather tests. There are rooms in the building for the storage of concrete materials, some of which have been under observation for ten years. In other rooms specimens may be "cured," as the concrete men say, under exactly the right atmospheric moisture. There is a refrigeration chamber where the effects of extreme cold may be watched. There are compression and testing machines, one of which has a maximum capacity of about 300,000 pounds. In addition there are chemical and physical laboratories; for concrete

research requires the assistance of the chemist and the physicist.

As stated, the greater part of the fundamental scientific work is done at Washington. There McMillan has a scientific staff of six working under a fellowship under the direction of Dr. P. H. Bates of the Bureau of Standards and Dr. R. H. Bogue for the Portland Cement Association. The scientific staff detailed to Washington is engaged, at the present time, in investigations into the atomic structure of cement. Here the work is precise, exact, and detailed; studies are being made to determine the nature of the transmutation which takes place when, at a temperature of 3000 degrees, the shale and limestone of the raw materials are changed into cement clinker.

At Washington Dr. Bogue and his associates are charged with learning how each atom in the mixture going into the kilns links its forces, augmented by the tremendous heat of the furnace, with those of other atoms to form new combinations of enormously increased potential energy. They have likewise devoted themselves to learning what happens when water releases this increased energy—as it does when mortar and concrete are made.

"It is not due to any 'divine curiosity,'" Dr. Bogue has said, "that the atoms of cement are being so closely scrutinized. For, by knowing how the atoms behave in the formation of compounds and in

the breaking up of compounds, a tool is at once available by means of which a positive control can be exercised over the product."

Sooner or later, into the conversation of every industrial explorer will creep those words, "positive control." It is what the metallurgist looks for, as he shrinks back from the heat of the small electric furnace in his laboratory. It is what the radio man seeks to achieve through vacuum tubes that are powerful enough to silence static. In cement research, "positive control" enables the chemist to predetermine the way in which the several groups of atoms shall behave, the combinations they shall make, the speed with which the combinations shall be formed. In other words, the chemist becomes the master of his atoms. The combinations most effective for the ideal concrete can be worked out. Does some railroad wish further to experiment with a concrete roadbed or concrete ties? The chemist is told to evolve, if he can, a concrete which will stand the terrific shocks of railroading. And he begins his work with the atomic structure of the raw materials.

There can be no hurrying about the work at Washington. The laws of science are not evolved overnight. When the research activities were first started some members of the Portland Cement Association, no doubt, wondered why they should be called upon to support the work of academic scientists who seemed never to be in a hurry, never to be depressed

because an investigation led nowhere, never to give a thought to dividends or profits. But it was because the methods of true science were insisted upon that so much was accomplished in the end and to-day most of the members of the association, only half-appreciating how it has been accomplished, agree that the money was well spent in view of the result.

An example of the results of research in concrete is found in the so-called "water-cement ratio law." No other discovery, probably, has had such far-reaching effects. Simply stated, the law maintains that in the range of workable mixtures of concrete of a given set of materials the strength depends solely on the ratio between the quantities of water and cement—the less water the better. Practical cement men had known, of course, the destructive effect of large quantities of water. But this was the first statement of a quantitative relation between the water-cement ratio and the strength of the finished concrete.

The law was discovered by Duff A. Abrams who organized and first directed the work of the laboratories. And it is probable that even he failed, for a time, to appreciate its full significance. Six years passed before the profession accepted the law to the extent that it became the basis of a building specification. And in the meantime numerous additional tests had confirmed its validity.

The reader, hearing that economy in the use of

water provides the best cement paste, is doubtless puzzled by the recollection of "humid" rooms at the laboratory in which concrete is "cured," or allowed to harden. There is an element of paradox in the fact that concrete needs water during the period when it is "coming of age." Although water must be sparingly used at the time of mixing concrete, it is well to use large quantities during the days and weeks of curing. Concrete does not, as was popularly supposed, harden by drying. Instead, a chemical combination goes on, uniting the water and the cement to form a hard, impervious paste which holds the aggregate in a rigid mass. If the water in the paste evaporates prematurely, the mass does not attain its maximum potential strength.

Here again, the laborious methods of research were called upon to give scientific data in confirmation of practices evolved by men long familiar with the properties of concrete prepared under every imaginable condition. In and out of the moist rooms they went, some for days and others for months. A few of the samples in one of the rooms in the Chicago laboratory have been under study for ten years. Out of it all came the verdict—moisture is essential for proper curing. And to-day, when a new highway reaches out to better transportation between two cities, pools of water are kept standing on the green concrete paving.

There were, of course, numerous other problems.

Into concrete go cement, water, sand, and stone. The first two comprise the paste or binding material. The last two, which are inert and undergo no chemical change, add bulk to the mass, improve the quality and lower the cost. But if sand was to be used, was one sand just as good as any other? The research workers attacked this, as a minor problem, and learned that certain sands were wholly unfit for the work. Cleanliness in mixture, they learned, was essential to maximum strength.

"Most of these facts," McMillan admits, "were nothing but common sense. The proof, however, was lacking until the laboratory produced the evidence, which was accepted and recognized by the concrete craftsman. This resulted in the evolution of a simple, accurate procedure in concreting which made possible an immense improvement in construction conditions and an actual saving in costs. But the clairvoyant common sense of the craftsman was the corner stone of the science of concrete."

C. E. Skinner

CHAPTER SIXTEEN

WHAT'S WRONG WITH IT?

C. E. Skinner

THERE is an element of similarity in the stories of most of these scientists whose skill, industry, and mental fertility have won them posts as directors of research in modern industry. It is no exaggeration to say, for instance, that a large majority of them attended Massachusetts Institute of Technology. Nearly all of them, having achieved a measure of distinction in their work, graduated to become instructors and then professors of chemistry, physics or engineering. In many cases they taught for years, trained young men in the principles of pure science and then were lured away from the cloistered life of the university laboratory by some far-sighted industrialist who saw that competition made necessary the employment of some genius who could explore new fields.

Having abandoned their caps and gowns, their classrooms and their university laboratories, the ex-professors actually changed their attitude toward their work very little. They remained professors,

teachers or instructors. Their students consisted of younger scientists, engaged as assistants. They were still men entirely uninterested in such important (to the industrialist) problems as mass production, dividends, costs, and cheaper operation. They followed elusive scientific trails and if these chanced to lead to lucrative discoveries, as very often they did, the research director was pleased but not greatly impressed. They resented frequent demands from the management that the laboratory give attention to plant operating difficulties. They never lost their devotion to the cause of pure science and they longed, from time to time, for the dear, dead days of a university laboratory where profits were unknown and where knowledge for its own sake was the sole objective.

The story of C. E. Skinner, who developed the research laboratories for the vast plants of the Westinghouse Electric and Manufacturing Company, at East Pittsburgh, Pa., would be interesting, if for no other reason, because of its difference. Skinner is, of course, a university graduate. But he did not go to M.I.T. He has never taught. His mind is strictly practical. He believes, and says so, that the duty of industrial research is the creation of greater profits. Skinner is as thoroughly scientific in his viewpoint as any of his fellow directors of research and is constantly called upon by them and by universities throughout the country to give lectures on

the most abstract problems which rise to taunt and bother the electrical industry. His attitude, however,—unless I am greatly mistaken,—is that it is his job to make things work. What's wrong with it he has been asking himself for the last forty years? Why doesn't it work? How can I make it work? Let me study this thing, he adds, and in time I think the difficult situation we find ourselves in can be cleared up. And in time, one might add, it is.

One of Skinner's first assignments,—as unique as any in the long history of research,—came during the early days of electric trolleys. Then, as ever since, he was in the employ of the Westinghouse Company and was sent to New York to assist in the electrification of the horse-drawn trolley lines. One morning, after a number of the lines had been equipped with power, a local superintendent named Schmid sent for the young research man.

"Skinner," he said, "I want you should find out the resistance of a horse?"

"The resistance of a *what?*" the youthful electrical engineer asked.

"A horse," repeated Schmid. "We keep getting complaints that horses are getting hurt on our line—truck horses that are driven over the tracks. The ground current is too strong, or something. Or the horses are too weak. Anyhow—go down to our stables and find out about it?"

Fortunately, Skinner had been brought up on a

farm and had no terror of horses. So he went to the stable and began his work.

"Electrification," he recalls, "was taking the place of the former horse-drawn cars and frequently the tracks were not in much better shape than before the motive power was changed. Rail joints were not bonded and complaints had come in that horses were injured or even killed by stepping on these rails, particularly when a horse happened to span the joints between the two rails.

"The job of measuring the electrical resistance of a horse seemed, when Schmid first told me to do it, something of a baffler. You remember, of course, that this was many years ago. I started down toward the stable with misgivings. The big, stolid truck horses did not seem exactly skittish, though, and I screwed up my nerve to the point of starting the experiments. First, I arranged to measure the resistance from their front feet to their hind feet. I got the horse to stand on pieces of clean sheet iron placed on the dry stable floor and measured the ohmic resistance from the front feet to the hind feet. This was compared—the current was very weak, of course—with measurements made from one hand to the other of my assistant. And to our very great surprise the resistance of the horses was astonishingly low compared with that of human beings. This explained why horses had been hurt and killed when walking on the new electric lines although men and women

had crossed the tracks repeatedly without being hurt in the least.

"I never knew exactly what use the superintendent made of the figures I turned in but thorough rail bonding soon became universal. Bonding completely eliminated accidents of this kind, gave higher efficiency to the circuits, of course, and helped to prevent electrolysis through stray currents to water pipes and other conductors."

Skinner's history and his philosophy of research may be somewhat different from those of the majority of his brother research directors. However, he had the early and active curiosity so frequently found in the boyhood of men destined to enter research. He was born on a farm in the southern part of Ohio, attended school when it was not necessary for him to help with the work, and spent his spare money for tools instead of for candy. His father raised wheat which was ground by a local mill operated by water power and when Skinner was about ten years old he was one day taken on a trip to the mill, a trip he has never forgotten.

His father was inside talking to the miller and Skinner, a good deal of an animated question mark, looked about for something to do. While thus wondering, he saw the miller's cat—a calm, venerable, dignified cat—whose duties included keeping the mill free from rats and mice. Would the cat swim if tossed into the mill pond? The fascinating problem

was too much for Skinner to resist. He picked up the cat, gave it a mighty swing, and tossed it into the center of the pond just as his father and the miller, shouting in unison, arrived on the scene.

"I have never," Skinner remembers, shuddering slightly, "found it necessary to repeat the experiment. This particular cat swam out all right and certain other painful recollections tend to mitigate any desire I might have for further experimentation of this kind. I can remember distinctly, however, that I had no feeling against the cat, no particular desire to cause it inconvenience. All I wanted was to find out whether or not a cat could swim. As a matter of fact, I often tried experiments when I was a boy and most of them seemed to get me 'in wrong' in one way or another. The results were not always appreciated by my elders."

Skinner, obviously, was born with no silver spoon in his mouth. To put it into words more applicable to the career he was destined to choose, no private laboratory was provided for him by the wealth of his parents, no gilt-edged education, no opportunities to study in Germany under the great scientists of his day. The reason was that the Skinner family had no wealth. They were entirely sympathetic when, as he grew older, he made known his desire for a technical education. But it was necessary for him to make his own way and to provide the money by working after school hours. The first school was one

near his home in Ohio, a small traditional red schoolhouse where the A.B.C.'s were taught with the persuasion of a cane and where the beginners were frequently boys and girls as old as seventeen and eighteen. When Skinner was seventeen he was able to attend the Fultonham Academy where he received his initiation into the world of science. His first taste of it came from a text known as Steele's *Thirteen Weeks in Physics* and the following year, on account of financial troubles caused by the illness of his father, he remained at home and went back to the country school. There he did some work in mathematics and physical geography.

"During my entire college course at Ohio State, where I finally managed to matriculate, I found it necessary to finance myself, with only occasional help from father," Skinner has said. "I did this by working on the experimental farm, working in the university dairy, and doing various odd jobs. The really important work that I did, important because it had a profound influence on my career and my attitude toward science, was in the university machine shop directed by the professor of mechanical engineering. This, together with my mechanical engineering studies, enabled me to qualify as a machinist and after a brief period of work for the Columbus Cash Register Company, immediately following my graduation, I secured a place with the Westinghouse Company in the summer of 1890. I did not go to

them as a scientist, I knew virtually nothing, of course, about science."

Skinner was, in fact, one of the first men to experiment with cash registers and he might have built up a fortune in this way had it not been that the National Cash Register Company got ahead of him. While still in college he perfected a machine with a total adding device and after graduation he started to manufacture them. He found himself out of a job, however, when the larger company started to turn out the same device and he was glad to accept a job, at twenty-five cents an hour, with the Westinghouse Company. This was in 1890, and he has been with the same company ever since. Year by year his reputation grew and he became research director and, finally, assistant director of engineering with authority in all of the vast plants of the company.

To-day Skinner is an authority in his field and sometimes surprise is expressed that he never left the Westinghouse for some other company. Not long ago, for instance, he was attending a dinner of electrical engineers and chanced to sit next to a high official of the General Electric Company.

"Skinner," said the executive, "I've always wondered why you never came to work for the G. E. We've got a nice plant, too, and we do some research work up at Schenectady."

Skinner grinned. "Just before I got a job at the Westinghouse I wrote to you and asked whether I

could get one with you. You wrote back, *yourself*, and said that my application was on file and that I would hear from you when there was an opening. As far as I know there hasn't been an opening yet."

In the nineties when Skinner began his period of service with the Westinghouse Company, research, as such, was unknown. At least, those who were really engaged in that work did not call it by that name. The plant of the Westinghouse was then located in Garrison Alley, in the heart of Pittsburgh, and the equipment was crude and inadequate. Those were the days of pioneering in electricity, particularly in the transmission of high voltages, and the men who with Skinner conducted the early experiments had many a narrow escape from bad burns. To-day, at luncheon, they gather in the pleasant dining hall of the administration building at East Pittsburgh and recall those early days in the Garrison Alley laboratory. Nearly all of them can tell stories of the peculiar things that happened as they gradually stepped up the currents with which they worked. And all agree that the work they were doing was actually research of the highest order—although that name was then unknown. It was not pure science. It was science applied to problems that arose almost daily to trouble an industry being born. Skinner, of course, was in the center of it all. Still far from being old, he has personally watched an industry grow from obscurity to greatness. Then,

the transmission of several thousand volts was beset with danger; to-day, alternating currents of one and one-half million volts can be handled with safety and assurance.

Skinner remained a machinist, his first rôle with the Westinghouse, for only eight months. Then he was assigned to the Garrison Alley experimental laboratories where he began the testing of insulation. It was while thus engaged that he was assigned to the problem of determining the resistance of horses. Experiments looking toward the construction and installation of apparatus for the so-called "Pomona transmission" were beginning. Prior to this time power had been carried from a generator to a motor with a line pressure of about 3000 volts. Now it was proposed to have a constant potential transmission involving 11,000 volts with step-up and step-down transformers. These had to be insulated with oil, and Skinner began a long series of experiments to determine the quality of oil to be used and to work out the design of the transformers. In preparing for this it was necessary to build testing apparatus capable of giving voltages of 35,000 to 50,000. This was far beyond anything previously attempted in the Westinghouse organization and the final testing was undertaken by Skinner under the direction of the chief electrician, Charles F. Scott.

By now Skinner's career as a research worker was definitely under way. But the practical was still

uppermost in his mind. For years, at any hour of the day or night, his telephone would ring and the superintendent of some far-distant power house would say:

"No. 3 has broken down. There's something wrong. Why won't it work?"

Skinner would jump from his bed, for all the world like a fireman, and hurry to the train. Sometimes he traveled for days to reach the scene of the breakdown. Sometimes he arrived in the middle of the night in the dead of winter at isolated power houses where transformers had burned out. It was his job to determine causes and to remedy the trouble. In the meanwhile he was becoming an authority on insulation and the transmission of high voltage alternating currents. Thus engaged, he observed a phenomenon now familiar to all workers with high voltages, but then new: that brilliant flashes were caused by placing a piece of thin insulation like mica between two electrodes and impressing on it a high voltage alternating current. A description of this phenomenon constituted Skinner's first technical paper, the forerunner of many that he was to write and deliver before engineering societies during the next twenty-five years. Before 1900 high tension electricity was still a mysterious novelty, and Skinner's demonstration of his mica phenomenon aroused widespread interest at the World's Columbian Exposition.

In 1895 the Westinghouse plant was moved from Garrison Alley to East Pittsburgh and the first of the many large buildings now dotting the valley was erected. Skinner was placed in charge of the newly organized engineering department and was ordered to begin a systematic study of insulation and the creation of insulation specifications to be used in the building of apparatus. The research point of view, it can be seen, was beginning to grow in the Westinghouse plants. But it was far from matured. Not long afterwards Skinner asked for permission to engage a chemist and his request brought startled protests from some of the company officials. A chemist in an electrical plant? What did a chemist know about electricity? Skinner succeeded in convincing them that a chemist could make himself useful and in due time the request was granted. When the chemist arrived, however, he found that no provision had been made for an assistant to help him in the laboratory.

"Where," he demanded, "is my bottle washer? I have always had a bottle washer."

Skinner was forced to admit, sadly, that the board of directors would grant no appropriation for a bottle washer.

It was years before a genuine research department was established, although the experimental work in the laboratories continued and the business of the Westinghouse Company doubled and quadrupled with the years. Skinner, of course, continued his

missionary work and by 1916 had won his objective. A research laboratory was established and he was placed in charge. His had been the responsibility of developing it and until a few years ago when he was made assistant Director of Engineering for Westinghouse, Skinner was in command.

"In the location and equipping of the laboratory," he has said, "it was considered essential that it be in a building apart from the regular factory, manned by the best experts who could be obtained and empowered to carry on the more fundamental work in chemistry, insulation, magnetic work, heat problems, ceramic problems, fundamental physical testing, and staffed with a certain percentage of men whose time could be given mostly, if not wholly, to theoretical studies in the general field in which the company was interested. It was further agreed that the research laboratory should have the closest possible coöperation with the Material and Process Engineering Department. This policy has been carried out. The success of this laboratory, under the present direction of Mr. S. M. Kintner, has been extremely gratifying to me since I was more or less responsible for its existence."

The laboratory, now manned by a large staff of physicists, chemists, and electrical engineers, is located on a hill across from the main buildings of the Westinghouse plant, in the valley. The visitor to East Pittsburgh, having been escorted through

huge factories with their ponderous machines and thousands of workmen, finds it a little difficult to realize that the laboratory is, to a large degree, the nerve center of all this activity. Here are studied the fundamental principles of electrical science and here are solved those problems which would otherwise slow down the wheels of the plant itself. It is impossible, of course, to enumerate all of the products of Westinghouse. The company manufactures a vast variety of machinery, appliances and apparatus; from the largest dynamos and motors to the tiniest measuring instruments. There are built transformers which handle thunderbolts, and meters which measure a hundred-thousandth of a volt. Motors, meters, lamps, electric furnaces, electrical fittings, transformers, insulators, sewing machines, and a host of other devices are made. You walk through the gigantic buildings and you come to a half dozen electric locomotives being finished. They stand on a track as they are being built and when finished will be hauled to the main line of the Pennsylvania Railroad, which runs through the yards of the plant, and sent on their way to the Rockies or to West Virginia where they will haul fast passenger or heavy freight trains across the mountains.

The products of engineering? To an extent they all are, of course. But in each case research played its part. Many of these products required special metals. For almost all of them insulating materials

had to be perfected. Often there were electrochemical obstacles to be hurdled. Some of the problems were solved in short order, but in many cases long and discouraging hours of work in the laboratory were necessary. It is safe to say that not a single product, even among all the thousands in the Westinghouse plant at East Pittsburgh, has been untouched by the magic wand of organized research. Some owe their very existence to the patient men working with test tubes, galvanometers, instruments that measure the faintest electrical impulse, scales that drop beneath the weight of a feather's rib.

Now over sixty, Skinner's life has been devoted to the development of research, to the perfection of the products that research has made possible. He is a quiet, unassuming, mild, and pleasant gentleman who seems to feel that his work has been without particular distinction but who finds satisfaction in the progress made by the electrical industry in his lifetime. Recognition has come to him in full measure during the past decade or so. He has been manager and vice president of the American Institute of Electrical Engineers and has served on many of its committees. For many years he has been a member of its standards committee and at one time was on the board of directors of the American Society for Testing Materials. Through his activities on committees engaged in standardization, the influence of his work has been felt internationally as well as in the United States.

Since 1920 he has not missed a single meeting of the International Electrochemical Commission. From 1895 to 1915 he did a good deal of traveling for the Westinghouse Company abroad and in this way visited most of the countries of Europe; he once made an extensive survey of the electrical plants in Mexico. In connection with insulation he coöperated with the National Board of Fire Underwriters and was one of the engineers who assisted in the formation of the National Electrical Code and the National Electrical Safety Code.

Primarily, it is true, Skinner is a disciple of research. The same influence which prompted the boy to toss an unoffending cat into a mill pond controls the man. There is, though, one difference: the man would not begin the experiment unless it had some relation to a problem demanding solution.

"As a rule," he said in a recent address, "the principal function of the industrial research laboratory is to develop new and useful products and processes for the industry it serves. In some cases, its main function may be, in coöperation with customers, to develop new uses for products already established. The laboratory may be established for the main purpose of improving manufacturing processes, or aiding the development of new devices and applications based on some outstanding invention which provides the main product of the manufacturers served. Most

industrial research laboratories find that an inevitable part of their work consists in curing and anticipating troubles either with the product, the processes involved or in the application of the product."

CHAPTER SEVENTEEN

SCIENCE IN THE BREAD LINE

Harry E. Barnard

Those who lament the passing of mellow beer may find partial consolation in the fact that the closing of the nation's breweries by the Volstead Act contributed to the improvement of a more vital household necessity—the loaf of bread. It is a commonplace of history that apparently unrelated events affect each other profoundly. The wheat and sugar shortages of the World War taught the American baker new methods of efficiency and economy. And the Volstead Act enabled the American Institute of Baking to obtain, in 1920, a perfectly equipped laboratory once operated by the brewers. Then began an intensive program of scientific research with Dr. Harry E. Barnard, president of the Institute, in charge. Scientists, many of them theoretical men who had never seen a loaf of bread being baked, started a long series of experiments.

In the years that have passed the baking of bread, once limited to the home kitchen or the small corner bakery, has become Big Business and the newspapers

carry headlines hinting at $500,000,000 "mergers." The result of it all is that the 1928 model loaf begins to approach perfection. It is more nutritious, more attractive in appearance, and more wholesome.

The newer American industries, such as telephony, automobile, and radio, were among the first to see that a research program was certain to bring dividends both to the public and to the industry itself. The older ones, like steel, textile, leather, and baking, were content for many years to follow the familiar paths. Why bother about science, their managers and presidents and boards of directors seem to have asked? The old methods had been good enough. So with baking. Its development had been infinitely slow. Dr. Barnard has pointed out, for instance, that bread baked a few decades ago was made in virtually the same manner as that of thousands of years ago in the valley of the Nile. Changes were few. Ten centuries ago the baker ground his meal and baked cakes in special ovens. During the next few centuries he improved his processes a little—sifting his flours and arching his hot hearthstones. One day he made up too large a batch of dough and left some of it unbaked. Thus it became food for yeasts, and the next bake that he tried turned out to be leavened bread. But the baker of fifty years ago used methods little better than those of his remote predecessors. He no longer, it is true, ground his own meal. But he still molded his dough by hand.

His ovens were in many ways identical. He was a craftsman-baker, doing in his small shop the same work that the housewife did in her primitive kitchen. The only important difference between the shop and the home was that in the former men took the place of women, that larger quantities were made up in a batch, and that the ovens would hold a hundred loaves instead of a half dozen.

It was not scientific research, but machinery, which brought the first important changes to the baking industry. During the last decades of the nineteenth century the use of machinery in bread making became common—and in a few years the industry made more progress than during its entire previous history. The power-driven dough mixer with its strong flexible arms of steel took the place of the brawny baker, who had mixed his doughs by sheer strength, and mixed it better! Then, for the first time, baking began to develop as an industry. The day of the skilled craftsman was drawing to a close. Flour went into storage by hoists and elevators and was sifted and blended by machinery instead of hand-shaken sieves. The dough room became a special chamber where fermentation took place under ideal conditions, where temperatures and moisture variations were controlled. Ovens were greatly improved, and machinery was installed which wrapped each loaf in waxed paper. Women no longer felt it a mark of slatternly housekeeping to buy bread at

the baker's, and husbands began to forget the bread "that mother used to make." In the modern bakery every process is automatically controlled and mechanically registered. The formula is always the same. The heat in the ovens is always uniform. The number of revolutions of the mixer is predetermined. Standards of cleanliness impossible even in a model housewife's kitchen become commonplace.

Picture the contrast between these modern bakeries—where chemists and dietitians write the recipes, electric switches control the ovens, automatic machinery transforms barreled flour into hermetically sealed loaves of bread without touching human hands—and the corner bakery of the nineties. Put the old-time baker in the line of spic and span, white uniformed employees of this new industrial convert to the creed of mass production and you will be convinced that here is indeed a striking picture of progress. And yet you have but witnessed the mechanical evolution of an industry. The underlying principles and basic processes are the same. The substitution of machines for hand labor, a speeding up of production, larger volume of sales, and quick turnover merchandise, it is true, but technically no fundamental change in process was made. What the baking industry needed, if it was to take its place in the present-day family of science-aided industries, was not evolution by slow experience but revolution by research. **It came! In** the guise of an unwelcome by-product of a social

revolution—an emergency in which all industries, particularly foodstuffs, were shaken to their foundations. The emergency measures of War!

"If necessity is the mother of invention," Whitney once declared, "then research are its parents!" And during the World War research mothered a new brood of infant industries which in later days call out in raucous challenge to their less fortunate brethren.

It was, then, the World War which forced the baker to use scientific methods, as distinct from mechanical, to improve his product. For many years the chemist had helped the miller to buy wheat. He had evaluated flour by weighing its ashes. His assistance had, though, been comparatively unimportant. Then came two years of war, "crowded with enough grief to make a lifetime miserable," Barnard has said. The baker turned his shop over to the Food Administration. He learned that bread could be made without flour—or with very little of it—and that there were other ways of feeding yeast than with sugar. He found that lard was not a necessity, and that relatively poor materials, properly used, would make excellent bread. He also learned that he could make bread which would not get stale overnight and this was important because during the war bread had to be conserved. They were not pleasant years, those of the war, but they taught the baking industry a good deal. Most important of all, they taught the

leaders that the industry must be rebuilt on a foundation broader and more scientific than that which had been slowly created during the years of craft effort.

In the spring of 1918 a committee of one hundred bakers took steps leading to the creation of an Institute of Baking which would develop a program of research for the benefit of both the baker and the public which he served. By 1919, under the leadership of George S. Ward who was president of the American Bakers Association, the Institute had been established. Barnard, long an expert in food problems, and food administrator for Indiana during the war, was placed in charge. Money poured into the research endowment fund and for the first time science had entered the baking industry.

Let us return, however, to the Volstead Act and its unintended benefit to baking. The first laboratories of the American Institute of Baking were opened by Barnard in Minneapolis, the center of the milling industry. It soon became apparent that the industry could not be fully served until schools had been established for the training of young men intending to become bakers. Meanwhile the allied industry of brewing—allied to baking since grains were used in both—had been legislated out of existence. In the days of its prosperity brewing had become a fine art and the products of the brewers' vats had been controlled by chemists and biologists.

Master brewers were graduated from special schools and chemical laboratories! Technically trained brew masters passed on the raw materials, the fermentation processes, and the quality of the finished product. A procedure worthy of emulation!

Barnard felt that the bakers must pattern their development work on the example which had been set by the brewers and he was delighted, therefore, when he learned that the Wahl-Henius Institute of Chicago was being offered for sale. This had been the leading scientific station for the brewing industry, but after 1918 it had been forced to dismiss its chemists and close its doors. The American Institute of Baking moved in, employed new chemists, opened new schools. Beer gave way to the perfection of the staff of life. The day had come when mere mechanical methods of making bread were not enough. Bread itself was to be improved.

It is easy enough to imagine the scene at what had been the Wahl-Henius Institute. There was, I suspect, slight difference between the laboratory of the bread industry and that which had served for the brewers. There may have been still a slight aroma of hops and malt around the place. The chemists who had been called in wasted no time, however, in mourning and they cared, one is certain, little or nothing about the passing of brewing. What they first wanted to determine was the degree to which bread was imperfect as a food. It was, they tound,

incomplete. It was rich in energy value. It was the fuel which propelled the human engine. But flour consisted chiefly of starch and protein. It contained little fat, less mineral element and almost no vitamins. Vitamins? These were the mysterious substances, present in minute quantities in the natural foods which came from the earth, concerning which science had learned much during the past few years. They were found to be in milk and in fresh fruits. They had food values far greater than the fats and starches which dietitians had so long praised. The experts at the American Institute of Baking felt that the vitamin substances must be added to bread.

Dr. McCollum, one of the leading nutrition authorities in the world, working in the academic atmosphere of Johns Hopkins University and unhampered by the pressure of an immediate "dividend of research," had thoroughly plowed the field of pure science investigation of vitamins and their function in nutrition, and their value as an element in food. The foundation had been laid for applied or industrial research. A vast treasure house of fundamental data had been stored up for use as the raw material of the "factories of science" in industry—the research laboratories.

And so, in the laboratories colonies of rats were established. The rat, always viewed with horror by the baker because it stole the flour from his sacks, now came to his aid. Fed on the common varieties

of bread, the rats suffered from malnutrition. But when the dough had been mixed with milk the test animals flourished. Milk, the product of green grass and ripened grain, is rich in the very elements that bread lacked. If a bread rich in milk could be produced at a price cheap enough to be practicable, the problem would be solved. Fluid milk was not, unfortunately, economically available in large cities. Poor people, whose children most needed the essential vitamins, could not afford to pay higher prices for their bread. Thus chemists faced the problem of reducing costs. In time they were successful. Ways were devised whereby milk was concentrated near the source of supply and then shipped with little or none of its original water content. An enormous saving in shipping expense considerably reduced production costs.

Out on the Pacific Coast, for instance, a dairy industry was being established on irrigated lands, by Portuguese farmers. One of these dairy centers lies in the torrid San Joaquin Valley and there, soon after the bread experts had decided that milk was essential to good bread, a large plant was erected. The Portuguese farmers brought their milk to this central plant. Each day, in less than half an hour, the fat content had been separated and sent to San Francisco for use as cream and in ice-cream. What remained, the skimmed portion containing all the milk proteins—the valuable mineral salts and the essen-

tial vitamins—was taken into great chambers where the water was evaporated. The resulting dry powder was then packed in barrels and shipped through the Panama Canal to the centers of population on the eastern seaboard. It became possible for milk produced in California to become part of the bread sold to the children of mill workers in Massachusetts!

While the nutrition laboratories were working to produce better bread, as such, the chemical laboratories were finding ways to improve the manufacturing processes. An important development was the discovery of the value of dextrose, a sugar made from the starch of corn. Yeast feeds on sugar, and the baker, who wanted the yeast to grow in his mixture of dough, had used the expensive variety obtained from the sugar cane. At the American Institute of Baking it was found that yeast could more easily assimilate dextrose and that great economies could be effected. The substitution of dextrose for cane sugar was another approach to the problem of reducing production costs. The baker, by nature a conservative, was unwilling to make the change at once. The Institute had to convince him by demonstrations in the laboratory, and prove to him that it was practicable for the small shop. Finally proof was presented also that it was feasible in large-scale production. To-day dextrose is widely used in baking and millions of bushels of corn, which might

otherwise have gone to the market in the form of pork, now become part of the modern loaf of bread.

Back of all this work—actively in command, although given full coöperation by Ward and the other important bakers—was Barnard. He is a large and jovial gentleman with a neatly trimmed beard and a full head of hair that is getting a little gray. Like so many of the men whose lives have been devoted to research, Barnard is a chemist. There is nothing reserved about him, nothing aloof, nothing academic. He is accustomed to dealing with business men, with industrial executives, and with boards of directors. He wears self-assurance like the Elks' pin in his buttonhole. He was born in New Hampshire in 1874. After graduating from the New Hampshire College at the age of twenty-five, he became an assistant chemist at the New Hampshire Experiment Station. In 1913 he obtained the degree of Doctor of Philosophy from Hanover College and later became research assistant to Dr. Wolcott Gibbs at the U. S. Smokeless Powder Factory in Maryland.

Barnard seems to have had a particular interest in the sciences which had reference to food and in 1901 he returned to New Hampshire where he was first the state chemist and where he organized a drug laboratory in connection with the Board of Health. In one way or another his activities have been related to food and health ever since. A few years later he came into contact with the famous Dr. Wiley who

was then seeking passage of his revolutionary pure food and drug act. Wiley was much impressed with Barnard's ability and offered him a post as head of the government laboratories in Chicago and New Orleans. At about the same time, however, the Indiana State Board of Health asked him to direct a laboratory about to be opened. Barnard chose this job although the salary was not as high as in the Federal position. He acted wisely, as time proved. Within a few years he was State Food and Drug Commissioner for Indiana and was playing a large part in safeguarding the interests of the people of the state against adulterations and impurities in foods and drugs. It was in this capacity that he first came into contact with the bakers and won their confidence. Their faith in him increased when, during the war, he became Food Administrator for Indiana. Barnard was able to obtain coöperation from bakers and the other food manufacturers. He did not believe in enforcing standards by making arrests. Instead, he went into the shops and the factories and convinced the managers that it was to their interest, as well as that of the public, to improve their product. He was then, as he is to-day, kindly, jovial, and a good deal of a diplomat. He won respect, of course, by the fact that he knew what he was talking about. He was not a vague theorist suggesting useless and impractical ideas.

Barnard could, on occasion, show determination.

Early in April of 1910, as an illustration, he issued an order which required all bakers to wrap their bread. This was a revolutionary demand. The National Association of Master Bakers protested. It would ruin them, they declared. No such order had ever been heard of, they argued with some heat. The State Food Commissioner pointed out to them that it was to their advantage to wrap the bread and he quoted prominent bakers of other states who had agreed with him. On this occasion he lost out momentarily. His order was suspended. But time showed him to have been right and the controversy brought his work to the attention of the bakers throughout the country.

The reasons for his selection to take charge of the research work of the American Institute of Baking was obvious when, in 1918, the bakers made their plans to create that organization. For Barnard, in addition to business sense and his ability to mix with men of nonscientific mind, had demonstrated his knowledge of science. Like his fellow members of that small band of workers in science known in the fraternity as "The Fifth Estate," Barnard is goaded by eternal questioning, impelled by curiosity and a lively imagination, and is lured into speculation of the future.

He believes that the present economic plight of American agriculture is directly responsible for the intensive chemical research on food problems. Chem-

ical knowledge is advancing to the point where the ultimate solution of the agricultural problem may be the abolition of the present system and the cause of a radical alteration of human nutrition. For according to his views, "standing room, not food supply, is the real limit of population on this earth," if the chemist, not the farmer, is pushed to produce the food supply.

Speaking at the sessions of the American Chemical Society Institute, recently held at Northwestern University, Barnard projected a graphic picture of the world's food supply—on the screen of the future. He carried the conviction to his audience that the chemist with the apparatus and methods of science will displace the familiar figure of the farmer with the plow. The chemist will play the leading rôle in the drama of existence in that distant day.

Barnard said, in part: "The chemist is impatient when he hears the Malthusian doctrine discussed in terms of wheat acreage, sugars, and fats, for he is confident that when the fertile acres of earth do not produce crops sufficient for man's needs the chemist can synthesize them in his laboratory. Indeed he is already doing that.

"When the need comes the chemist will convert the light of the sun and nitrogen into food for the human family. Thirty men working in a factory the size of a city block can produce in the form of yeast as much food as 10,000 men tilling 57,000 acres under

ordinary agricultural conditions. By what right do we assume that millions of years from now man will be the same kind of organism he is to-day? He may live differently. He certainly will eat differently. The taste of good bread and meat may have been forgotten for ages, but his metabolic processes will go on just as satisfactorily as to-day."

Chas L. Reeves

CHAPTER EIGHTEEN

DISARMING DYNAMITE

Chas. L. Reese

TEN years ago, when the war ended, the United States government found itself the unwilling owner of a commodity of dubious value—millions of pounds of the deadliest explosives ever made. The bulk of this dangerous property was trinitrotoluol (TNT) and picric acid. Obviously it was useful enough to kill and to maim enemy troops, to blow up enemy cities, and to spread destruction with the quickest possible dispatch. But what could be done with it now that slaughter and destruction were no longer called for?

Nobody wanted it, nobody knew what to do with it, and, particularly, nobody wanted it stored near where they lived. Public opinion, remembering disastrous munition factory explosions, demanded that it be dumped into the sea. And the government, although it deplored the tremendous economic waste involved, became half convinced that this might be the best plan.

In the meantime a group of determined chemists,

under the guiding hand of one man, were working out a plan to beat the chemical swords of war into the plowshares of peace. Blending chemical compound with chemical compound they found a way to make the deadly weapons of war safe for peacetime use. Through their work a new industrial explosive, Sodatol, was evolved from the wartime stocks. Factories were set to work to make the transformation, and as fast as it was completed the government placed the new explosive on sale. The result was that five years after the war had ended many of the nation's farms were being tilled, and many of its highways were being built, with the aid of munitions that had been made to help win the war.

It was particularly fitting that the man who was the dominant figure in this drama of the disarmament of wartime explosives, was also the one who largely directed their development and manufacture. That man was Charles L. Reese, then chemical director of E. I. du Pont de Nemours and Company. For nearly fifteen years before the start of the war Reese had directed his staff of chemists in their search for the most efficient explosives that could be devised. During that time he had built up and trained a chemical department of enormous size; at one time during the war more than 1200 trained chemists were under his command.

When the war came, the organization which Dr. Reese had builded with such care went into imme-

diate action. Long before the United States entered the war they were at work developing and directing in the production of the powder that fed the Allied guns. They did their work so well that by the time the United States declared war the du Pont Company was the mainstay of the Allied cause, producing better explosives than had ever been known before at prices lower than men had dreamed they could be produced.

Tall, erect, with serious dark eyes peering out under heavy eyebrows set in a jutting forehead, Reese looks as well as acts the leader of men. His erect bearing, in spite of a somewhat portly figure, is a heritage of the time when he was a major as well as professor at the South Carolina Military Academy at Charleston.

When he is at work his eyes are serious and his mouth firm under the heavy mustache that is also a relic from professorial days when a beard seemed useful to impress scholars scarcely less youthful than himself. But when he is at ease and enjoying himself, the eyes twinkle and every feature relaxes into a genial smile. One feels that he has found keen enjoyment in life, on the whole—in the men he has met, the work he has done, and the changes he has seen.

His friends—and he has nearly as many friends as any man alive, scattered among the countries of the world—say that the greatest individual factors in

his success have been a broad and far-sighted mind, an ability to pick men, and a personal magnetism that makes them follow where he leads. His personal charm, they might say, is largely the product of a breadth of interest that has made him a connoisseur of etchings and a lover of fine books and music, as well as a scientist.

In an evaluation of his work "disarming dynamite," the work of making dangerous explosives safe in the hands of the workers in the fields, on the roads, or in the mines, might well be chosen as the central theme. For although his main achievement was in the building of the great chemical department of which he was the head for twenty-five years, explosives have played a large part in his career. He was one of the pioneers in the high explosives field; he remains one of its outstanding figures.

One of the great achievements of Reese and his chemists in the disarmament of dynamite, and one that has saved hundreds of lives was the development of nonfreezing dynamite. Through this single piece of research the most dangerous single factor in the handling of this explosive has been almost entirely eliminated.

When the old type of dynamite was used in cold climates, it had to be coddled as carefully as a child. For when it froze, and that was frequently enough, it had to be carefully thawed out before it was fit for use again. There were a dozen ways to thaw it out,

all of them dangerous. The one most frequently used by the miners was to put the sticks in a can of water over an open fire. But often the miner who tended the fire was careless. He let the fire get too hot, and the dynamite exploded as he watched. Or perhaps he might warm it up in his boot, half unconscious of the peril he faced and for his momentary lapse he paid with the loss of his eyes, his life, or with a shattered body.

For years the du Pont chemists at Reese's command labored to find a dynamite that would not freeze, that would not need to be dangerously thawed. Early in the work a low-freezing dynamite was developed containing trinitrotoluene and similar compounds. This was a great advance, but later a practically nonfreezing dynamite was developed by a modification of the glycerine from which the nitroglycerine was made, thus eliminating the dangers incident to the handling of frozen dynamite.

One other great contribution to the safety of mining dynamite has been made by Reese and his chemists, the so-called "permissible" explosives, and, as in the case of the nonfreezing dynamite, countless lives have been saved as a result of their painstaking work.

More than twenty-five years ago, when Reese's attention was first attracted to the problem, explosions in coal mines were quick, frequent, and ghastly in their toll of lives needlessly lost. In many coal

mines, the underground caverns were filled with fine coal dust and inflammable gases. All that was necessary to set off an explosion of fatal effect was a tiny flame, and the powder then used in mining gave off a flame dangerously long. Miners set it off with the almost certain knowledge that sooner or later a too long flame would set off the gases and dust of the mine and send them to their doom.

Reese, then at the beginning of his career as a director of research, attacked the problem with characteristic thoroughness. Europe and America were scoured for information that might help to develop a dynamite safe for use in coal mines. And in the end he and his chemists were successful in their search, for powders without the fatal flame were developed and made. According to the United States Bureau of Mines, not a single explosion from gas and coal dust in mines has occurred in cases where the "permissible" explosives, developed by the men who disarmed dynamite, were used.

The story of Reese's rise to the directorship of one of the greatest chemical industries the world has yet known is as dramatic in its way as the story of the fight to make explosives safe for the masses. At eighteen a lean, serious-eyed student at the University of Virginia; at thirty, a professor of chemistry at a small Southern College; at forty, a director of the chemical department of the du Pont Company, an organization which grew under his

direction to be one of the largest of its kind in the world, employing 1200 chemists—this tells briefly the story of Reese's rise.

But underneath this bare outline lies a story of painstaking effort that goes far to show how the student of those early Virginia days forced his way to the top of his chosen field. Reese was born during the stirring days of the Civil War, the son of a well-to-do Baltimore merchant. There were eight in the family, five brothers and three sisters; after the father died, when Reese was barely thirteen, there was scarcely enough to take care of them all. The eldest, Frederick, now the Protestant Episcopal Bishop of Georgia, was then just preparing to enter the ministry, and could offer little help.

The public schools of Baltimore were the lad's early training ground. When he entered Johns Hopkins University at seventeen, the modern science of chemistry was just at its dawn. The wonders of the science intrigued the boy, its mysteries piqued his active curiosity, and the next year, with the aid of his eldest brother, he entered the University of Virginia to follow his chosen pursuit.

Three years at the University strengthened his determination to become a chemist and convinced him that Germany would be the best place to pursue his studies. He was fortunate enough to be able to borrow money to continue his studies, and that summer he sailed to take up his studies in Europe.

At Heidelberg he fell into the hands of Bunsen, then in his prime, and in the incredibly short space of a year and a half completed his work for the degree of Doctor of Philosophy. Later he went to Göttingen to study under the celebrated Victor Meyer. Since that time he has received many academic honors: the Universities of Pennsylvania, Colgate, and Delaware have conferred upon him the honorary degree of Doctor of Science, the University of Virginia has made him a Phi Beta Kappa. But it is doubtful whether he will ever again experience the thrill that was his when the officers of the University of Heidelberg decided to award him that first degree, although he held no preliminary degree of any kind, and although he had been resident at Heidelberg only a year and a half.

On his return from Germany he drifted into teaching and followed it faithfully for fifteen years at Johns Hopkins, at Wake Forest College, and at the Citadel at Charleston. Those quiet years in the calm of college cloisters were the most productive of his entire career so far as individual research was concerned.

Best known of all his work during these formative years was a study on the origin of the Carolina phosphates. How these phosphates had originated had been a source of contention among geologists for years. Reese, with his knowledge of chemistry, was

able to solve it and to offer an explanation that stands undisputed to this day.

But during all this time Reese was comparatively unknown outside of his own field. Industry had not yet begun to call upon the chemist to solve its problems, and there were few openings for the young chemist, except as a teacher or in pure scientific research.

So it was not until the beginning of the new century that Reese stepped out of his rôle of teacher and began his brilliant career as a director of industrial research. The contact process for the manufacture of fuming sulphuric acid was in its infancy at that time, and although it had its inception in Germany, many difficulties arose in transposing this process to the United States. The great German industrialist, Otto Witt, in a speech made before the International Congress of Applied Chemistry in London in 1909 made the statement that industrial processes brought from one country to another almost invariably lead to difficulties, citing as an instance, the coal gas industry taken from England to France and Germany. This was also the case in the contact process brought from Europe to the United States. So, in 1900, Dr. Reese was called to leave his professor's desk to join the forces of the New Jersey Zinc Company where the problems in connection with adapting the contact process to use in this country

was assigned to him, and he was eminently successful in the full solution of these problems.

It was at this time that he experienced what he calls one of the most dramatic moments of his career. The contact method for the manufacture of commercial fuming sulphuric acid, which he had developed to a high degree, was then comparatively unknown, so he was asked to demonstrate the process in the Chemists' Club in New York.

The room in the club was crowded with the most important chemists of that time. He had set up a miniature plant made primarily of glass so that the whole operation could be observed, and in this plant he produced solid sulphur trioxide and fuming sulphuric acid before his audience. That was the first time this process was ever demonstrated before an audience in the United States. When the pungent sulphur trioxide rose into the room announcing the success of the experiment, every man in the crowded room pressed forward to congratulate the experimenter.

Less than a year after Reese began his career with the New Jersey Zinc Company, the du Pont Company found in him the man they wanted to develop the great chemical possibilities of their company and asked him to come to them to build up a chemical department. Reese assented, and it was this move which proved the turning point in his career, for it proved that, capable as he was as teacher and ex-

perimenter, he was a far more brilliant executive and leader of men.

The story of the man would not be complete without a record of his achievement as a builder. For he is primarily a builder, the man who almost alone organized the great du Pont research organization which is in so large a measure responsible for the foremost position which the du Pont Company holds in American chemical industry.

When Reese entered the du Pont Company in 1901, it already had nearly a hundred years behind it as a maker of explosives. He was almost the whole chemical department; the army of 1200 chemists that he commanded during the war was as yet undreamed of. Beginning with a study of explosives, he saw the possibilities offered by other allied lines, built laboratories, and picked chemists to study them and develop their possibilities.

The constantly expanding research organization under Dr. Reese had proven to the officials of the company that the tremendous advances in the explosives industry and the savings brought about in that industry by the application of scientific methods of control might also justify the company's expansion into other related fields. Consequently, as a result of their confidence in their research organization to carry them through, the Company branched out into other lines of manufacture including artificial leather, pyralin, pigments and heavy chemicals, paints,

lacquers, artificial silk, and the like. He was among the first to sense the possibility of an American dye industry and he led in fostering its development. And by the time he retired from active work as chemical director in 1924, the manufacture of explosives had become only a part of the widely diversified activities of his company. Disarming dynamite, he left his company, still the greatest manufacturer of munitions in America but greater still as a maker of the instruments of peace.

But Dr. Reese's activities have never been strictly confined to the company of which he was for so long the chemical head. He was a founder of the Association of Directors of Industrial Research, and has long been a member and officer of the National Research Council. The high esteem in which he is held by his fellows is attested by the offices to which they have elected him: president for three years of the Manufacturing Chemists' Association of the United States; twice president of the American Institute of Chemical Engineers; director of the American Chemical Society, and chairman of both the Philadelphia and Delaware sections of the same society. He has been honored by election to membership of the American Philosophical Society, is a member of the Franklin Institute and various other scientific societies. He has recently been elected a vice president of the International Union of Pure and Applied Chemistry and has served his government as associate

member of the Naval Consulting Board, chairman of the Delaware Section of the same body, and as a member of the advisory board of the Chemical Warfare Service of the Department of War.

That rare combination, humanist and scientist, lover of the arts and leader in industrial research, Reese has carved himself a niche in the history of his time and a larger place still in the hearts and the minds of the men who know him. His work as a chemist has undoubted value; his work as an executive has been more valuable still. But by far his greatest work has been that of a man among men. As executive and leader, he remained always a teacher and trainer of men. As representative of his company abroad, he has become, as well, ambassador without portfolio.

CHAPTER NINETEEN

THE INSIDE STORY OF LEATHER

George D. McLaughlin

SEVEN or eight years ago a representative of the leather industry approached George D. McLaughlin, a tall, lanky, professorial-looking gentleman, and announced that his talents were needed in the development of a research program. The tanners of America, he said, had finally become convinced that their product could be improved (and the peril of competition from artificial leather met) only through the aid of science. Would McLaughlin, who had long been familiar with the industry but who had retired to academic life at the University of Cincinnati, assist them? Would he assume direction of a research laboratory?

The leather men had, as a matter of fact, numerous conferences with McLaughlin and each time they found that he had held very definite ideas regarding research. What did they mean by scientific research? he asked, at some of the early sessions. Where did they propose to establish the laboratory?

George D. McLaughlin

"Well," said the leather industrialists, in effect, "as we understand it research is a way to improve your product. You get a laboratory and put some scientists to work. You locate it in some city near the more important tanneries and the scientists work in close coöperation with the plant. When something goes wrong they fix it. We're told that in a short time large economies in operation will result. The research game pays dividends."

McLaughlin must have smiled to himself when he heard this sort of thing. At all events, he made it emphatically clear that their conception of research was far from his own. It was, to put it plainly, entirely erroneous. He would consider, he said, becoming a director of research for the tanners but he would accept the assignment only upon his own conditions. Any laboratory would be truly scientific. The work would be carried on as far as possible from the tanning centers. Time and money would not be wasted by attempting to adjust the day-to-day problems of the tannery. The laboratory would guarantee no profits. But it would probe deep into the fundamental problems related to leather and in due time much, and possibly valuable, information would be acquired. Greatly to their credit the tanners agreed to McLaughlin's viewpoint. They have never regretted their decision. Research—even this apparently theoretical research—proved successful and some years later most of the important leather men of the

United States gathered on the campus of the University of Cincinnati to dedicate a new three-story laboratory just completed. An elderly tanner was one of the speakers on this auspicious occasion.

"I didn't know anything about this research idea when it was started," he said. "I'm willing to admit that I didn't have much faith in it. But I'm here to say that the work done by Professor McLaughlin and his laboratory is saving me $100,000 a year."

The accomplishments of McLaughlin, which are obvious to all who have visited the finely equipped laboratory at Cincinnati, are the more remarkable because tanning, like baking, is one of the oldest industries in existence. And the conservatism of the industry, we have already seen, varies in direct proportion to its age. Tanning is a most important industry since its products are used for shoes, for harness, for machine belting, and for upholstery. Deprived of leather, man would be most uncomfortable, for his shoes would be far less perfect. Without leather many a vast factory would, at least temporarily, close its doors.

The tanning of leather was one of the first discoveries made by man as he evolved from the status of savage. For centuries the processes were cloaked in secrecy and the father who had been a tanner imparted them only to the son who would fall heir to the tannery. Every one knew that a piece of undried animal skin would soon decay if allowed to remain

unprotected. Decay was halted by treating with various substances, and the word "tanning" is derived from "tannin" a substance obtained from woods, barks, and roots when soaked in water; the fluid tanning liquor, converts fresh animal hide into leather as we know it. To-day various chemicals are used. The craft days of the leather industry were the result of abundant supplies of raw material and the fact that competition was mild. As industrial methods developed and competition became keen, the more progressive leather men realized that progress lay only in understanding the scientific laws behind the processes. Tanning, they were to learn, involved nearly every branch of chemistry. It was closely allied to medicine, since tissues and skins had to be studied. A mastery of bacteriology was essential and the miscroscope was to be an essential part of the laboratory equipment.

The willingness of the leather men to bow to the wishes of McLaughlin was due, in large part, to the fact that they had long been organized into the Tanners' Council of America for mutual benefit. An agreement was drawn up with the University of Cincinnati whereby it was arranged that McLaughlin, already a member of its research staff, was to be in charge. It was specified that no research except of a strictly fundamental nature was to be undertaken—a clause of the utmost importance since it meant that the individual corporations belonging to the Council

could not interrupt the work by asking for study of their separate problems. It was further agreed that the Council would furnish the funds for the work and that the results of all research would be freely published in reputable scientific journals. McLaughlin, it is obvious, had won on all points. In 1921 the work got under way. That it has been fundamental, rigorous, conscientious research is indicated by the fact that in the seven years that have passed the scientists have not yet reached the subject of tanning itself. They are still engaged in the preliminary work.

Why was McLaughlin chosen? Why was he able to impose his own ideas on these hard-headed, practical, money-making leather men? The answer to these natural questions is found partly in his history and partly in his personality. He is amongst the youngest of all the directors of research, having been born in 1887, and his story is distinctly novel inasmuch as he is virtually self-taught. Most of those who have followed research have been university graduates holding advanced degrees. McLaughlin, however, worked as a "laboratory helper" in a tannery, actually as a "bottle washer" before he had gone to college at all. He was born in Retort, a small village in Center County, Pennsylvania, and before he was twenty years old he was working for the tanning firm of Lees and McVitty of Philadelphia. Within a few years he was in charge of its chemical

work—and this despite the fact that he had never been inside a college laboratory. During those years, he mastered all the technicalities of tanning and in time he became an authority on leather and its processing. In 1913 he went to California to become chief chemist for Kullman, Salz & Co., another tanning concern. All this while he seems to have been disturbed by convictions that tanning was still far from a scientific industry. He felt that there was a definite need for research, and so, in 1919, he resigned his position on the Pacific Coast, accepted a much smaller salary, and became a research associate in the Department of Physiology at the University of Cincinnati. In that capacity he had an opportunity, for the first time, to study in a quiet and thorough manner the underlying chemical changes that take place during the curing of animal skins. And word of what he was doing, and learning, occasionally got back to the leather men for whom he had once worked and with whom he had been associated. Finally, in 1920, the Tanners' Council began to ponder the question of a research program. This came about through the missionary work of Dr. Harrison E. Howe, at that time chairman of the Division of Research Extension of the National Research Council and editor of *Industrial and Engineering Chemistry*. Dr. Howe was asked to address the leather men on the general subject of chemistry in the leather industry and to tell them, if he could,

what advantages might accrue to the tanners through following research. So convincing was his address that the Tanners' Council decided to begin, in a small way, a constructive program. Naturally, they recalled the earlier activities of McLaughlin and the work that he was doing at the University of Cincinnati. They asked him to assume command and he did so, as we have seen, on his own terms.

Obviously McLaughlin is no aloof scientist. He is, in fact, a rare combination of business man and scholar. The directing head of the laboratory of Cincinnati, McLaughlin is a good enough business man to hold the respect of the tanners at the same time. Tall, quiet, self-assured, he continues to insist that the laboratory give its attention to fundamental research alone. While serving as a research associate at the University of Cincinnati, McLaughlin had been completing the education interrupted when he was young and in 1920 he had received his master's degree. To-day his professional ability among research workers is beyond question. The author of many scientific papers, he is a member of the American Chemical Society, the American Leather Chemists Association, the American Institute of Chemical Engineers, and the Royal Society of Arts.

The laboratory of the Tanners' Council at the University of Cincinnati bears a striking resemblance, although it is much smaller, to the laboratory of Dr. Arthur D. Little at Cambridge. It has the same

air of compact efficiency. It was built for the particular purpose for which it is being used. There are three divisions to the laboratory—chemistry, bacteriology, and histology—and each occupies an entire floor of the three-story structure. There are private laboratories for the department heads and laboratories for the assistants and a limited number of graduate students enrolled at the university. The equipment has been specially designed and in many cases has been built under the direction of McLaughlin and his associates. The visitor to the laboratory is struck by its resemblance to that in a medical college and this is logical enough for, whatever the public conception of tanning, the relation between the curing of leather and the treatment of some forms of disease is not dissimilar. A physician is in charge of the biology department. In the chemical section the various solutions used as tanning liquors are experimented upon and new ones evolved. The bacteriologists study the activities of bacteria on hides and skins. The histology experts work with microscopes to get down to the fundamental elements in the structure of hides and leathers.

"The 'agreement,'" McLaughlin has said, in speaking of the arrangement made with the tanners when the research work began, "has worked in practice. During the early stages of the research there were, as would be expected, expressions of discontent from the less progressive, unimaginative members

whose understanding of fundamental conceptions was necessarily vague and who expected the tree to grow to maturity and yield a golden crop within a month after planting. Another well-meaning group felt the urge of offering suggestions of 'problems' demanding immediate attention without, of course, any conception of whether their suggestion had a scientific basis.

"When such misdirected, though well-intentioned efforts possess sufficient pressure, the scientist (whom the industrial group looks to for 'results') often loses heart, his enthusiasm is dampened and, realizing the futility of the situation, he simply resigns. Or, if he lacks sturdiness, he is overawed by the powers before him and begins a necessarily fatal compromise; he seeks to pacify one group by investigating some probably wholly unessential problem they suggest. Scarcely is this done when another group, or a particular corporation, not to be outdone, advances with a suggestion; the precedent has been established and he must meet their demand. In a comparatively short time the scientist's time and energy are divided between routine analytical or testing work and a feverish effort to placate the ever-growing disappointment of the industrial group which had been led to expect large increases in their dividends. Thus the undertaking which started with the martial strains of a brass band ends with the weak notes of a harmonica. The net result is that a substantial sum

of money has been wasted and nothing gained but disillusionment."

I cannot imagine that the difficulties which beset the path of the beginning research director can be better described than in the above quotation from McLaughlin. It is needless to point out that this particular scientist did not make these errors. No harmonica is sounding feebly through the halls of his laboratory building. Of the seven major operations in tanning the first is the death of the animal and the removal of the skin. It was at this point that McLaughlin began his work. He went back to the first fundamental and his associates learned everything that there was to know about this. Eventually work progressed to the second step; the curing of the skin. Again years of labor intervened. So with dehairing (the removal of hairs), preparations for tanning, actual tanning, and finishing. Nothing, as a matter of fact, has yet been done with tanning, for McLaughlin's scientists have not yet reached that point. So slow and so thorough is the research mind in operation!

To the layman the tanning processes may appear simple enough. Actually, they are complicated in the extreme. Science has still much to learn regarding the composition of skin and its chemical and physical behavior. A piece of iron is, in comparison, a very simple substance and the steel maker can work with a definite degree of certainty. Animal tissue, how-

ever, is made up of many different elements which may combine with each other in a vast variety of ways. The tanning scientist is really concerned with knowing: (1) the actual composition of the original skin; (2) how and why this composition produces the best leather; (3) what happens when a skin is tanned.

The difficulties are great but progress has been made. McLaughlin points out that it is now known that most of the changes which occur in skin from the moment of death until it has passed through the tanning process are the result of bacteriological action. That is why the laboratories at Cincinnati are crowded with microscopes and why, in each of the various rooms, there are ice-boxes where cultures are stored and incubators for their reproduction. It is known, also, that the changes in the animal skin begin immediately after death. Definite laws have been evolved for the curing of skin and these, given to the packers, have enabled them to produce skins which bring a premium at the tanneries. Great light has been thrown upon the dehairing process, and one development which accelerates this is so valuable that the laboratory has taken a patent on it. Any one can use the process, upon payment of a license fee. The revenues are applied to an endowment being raised to carry on the work.

McLaughlin has no patience with complaints, occasionally inspired by knowledge that an American

university is conducting research into tanning, that the nation's colleges are becoming commercialized. He feels that the university owes a definite debt to industry and that from the prosperity insured by industry come the funds which enables a university to continue its work. On the other hand, he has just as little patience with pseudo-research. The work done at Cincinnati is scholarly and conscientious. From time to time reports are published in reputable scientific journals so that any one interested may read of it. Visitors are welcome at the laboratory for the scholar-scientist knows no secrecy. Within recent years they have been coming from all parts of the world, for the work that McLaughlin is doing has gained international repute.

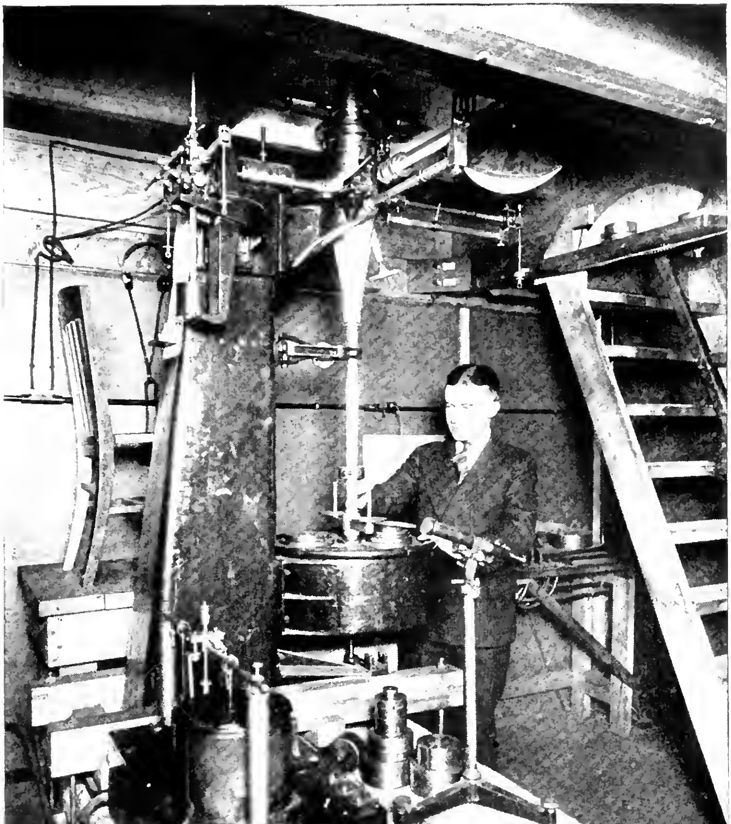
CHAPTER TWENTY

SCIENCE TAKES WINGS

William H. Miller

On a morning in the summer of 1911 most of the small boys of Shawnee, Okla., and not a few of the older ones, gathered at the edge of a small cliff on the outskirts of the town. A passer-by, seeing them, might logically have assumed that a circus was to pitch its tent on a nearby field. The theory was not entirely incorrect, but the circus, instead of being a vast affair with scores of performers, consisted of a single fifteen-year-old youth.

Calm and self-assured, the boy who was the center of attention shouted commands to a group of other boys who were carrying a huge contraption easily identified as a homemade monoplane-glider. "Bill" Miller, the hero of the moment, was Shawnee's leading aviation enthusiast. He had already pored over the existing literature on the science of aviation. He had studied diagrams and technical drawings in the *Scientific American* and other publications. And now he was ready to test his knowledge by sailing off from the cliff in the monoplane which he had built. As his



glider was being carried toward the edge of the cliff one or two more cautious friends begged him not to make the attempt. The drop, they said, was at least twenty feet! It was conceivable, to put as optimistic a front on the matter as possible, that the glider would not glide. Twenty feet constituted quite a drop. The monoplane was poised on the side of the cliff, with Miller propped in the framework, his feet on the ground. Two or three of his friends held the tail of the machine.

"Ready?" the pioneer called.

"Set!" his assistants answered. And Miller ran a few steps into the breeze and jumped off the cliff. His calculations, alas, had been at fault and the glider did not sail into the breeze according to schedule. Instead, it dropped like a plummet to the field below and when the boys above got down they found Miller, miraculously unhurt, extricating himself from the mass of sticks, cloth, and wire.

"Go back to your singing!" one of the boys yelled —a most unkind remark based on the fact that Miller was the village tenor and was being urged by his parents to follow a musical career.

He did not do so. Instead, he went back to his studies of the theory of flight. He has been at them ever since and to-day, not much more than thirty years old, he is director of research for the Curtiss Aeroplane and Motor Company of Garden City, L. I. No small part of the astonishing advance which

has been made in aviation is due to his careful, painstaking work in transforming theories into facts. The boy who tumbled off the cliff far out in Oklahoma has become an enthusiastic, dynamic, earnest individual who grows irritated when uninformed critics suggest that aviation is limping along on the "trial and error" method, that its engineers are really merely mechanics, and that as an exact science it is just a little beyond the pale. Miller silences these critics by taking them through the Curtiss laboratories, showing them the wind tunnel where numerous complicated tests are made, asking them to glance at a few sheets of calculations regarding "drift," "propeller-thrust," aerodynamic coefficients, and other mysteries. He concludes his argument by pointing out that the automobile industry, whose engineers evolved many of the first airplane motors, are now turning for advice to aviation. The infant industry is beginning to pay back its debt and automobile designers, seeking "streamline" effects which will cut down wind resistance, are experimenting with models in the wind tunnels which Miller has built.

It is appropriate that Miller should be a young man for aviation is one of the newest of sciences in an age when hardly a year passes without the birth of something startling and different in transportation, communication or industry. He looks, perhaps, even younger than he is—not unlike a junior assistant in some university or technical college. But he is,

withal, impressive enough, particularly when he is surrounded by the scientific apparatus down at the Curtiss plant or when such elderly grizzled pilots of the air as the famous "Casey" Jones of the Curtiss testing staff drop down from the clouds and call upon him for advice. Miller is rather short and stocky, although far from stout. He wears his hair neatly parted and brushed, neckties that are a little collegiate, and suits that are conservative. His eyes are keen and show an occasional flash of humor. He is popular among his subordinates, enough of a salesman to obtain appropriations for his work from practical-minded executives, enough of a mechanic to build his own models. Miller, whose study of aviation began with theory and who did not learn to fly for years, is practical enough to reduce his theories to a working basis. And yet he does not permit his connection with an industrial concern to dwarf his interest in the studies which he began as a boy out in the West.

Aviation is a young man's game and it is not without interest that two of the men who have aided materially in its development were small boys when the Wright brothers rose from the ground in December of 1903 in their crude heavier-than-air machine. The younger (he was not quite two years old). Charles A. Lindbergh, lived in Minnesota. The other, William H. Miller, was in Texas. Lindbergh was destined to do the flashier thing, to fly from New

York to Paris, to win headlines, to dramatize the conquest of the air. Miller was to become the patient laboratory worker who would test theories with the instruments of an exact science. In 1914, while Lindbergh was dreaming of the day when he would fly, an eighteen-year-old Miller was writing an article for *Aero and Hydro* under the erudite title "Locating the Center of Lift on a Biplane." It was the first of many technical articles that Miller has written and it is, of course, unnecessary to record that it thrilled him more than any other. He still has the issue of August 29, 1914, in which it appeared.

Miller was born in Paris, Texas, in 1896, spent his earlier boyhood on the farm of his grandparents near Clarksville in the same state and moved to Shawnee, Okla., when he was eleven years old. He received his first schooling in Texas and also had his first chance to experiment with tools. His grandmother, he recalls, permitted him to make kites, windmills, model engines, and sailboats as long as he would also build chicken coops and repair fences. He also found time to devise traps for the birds and small animals of which there was great abundance in eastern Texas. By the time he had moved with his father to Oklahoma, Miller was skilled in the use of tools and by then, too, he had become absorbed in the new science of aviation.

"I became interested," he remembers, "from reading of the exploits of the Wright brothers, Glenn

H. Curtiss, and others. Pictures of their machines which appeared in the *Scientific American* inspired me to build the glider which was wrecked when I was fifteen years old."

Miller was fortunate in that his elders, while somewhat dubious regarding the hobby of aviation, did not discourage his bent towards the scientific. By the time he had reached his senior year in high school he had gained a fair knowledge of elementary physics and his father, who was in the cotton business, bought him most of the aviation textbooks of the period. Among those over which he pored was an English translation of Gustav Eiffel's *The Resistance of the Air and Aviation* and the ponderous volume had a profound influence on his life. His father also permitted him to subscribe to most of the early aviation magazines and a high school mathematics teacher whom he recalls with gratitude, a Miss Ella Mansfield, gave him much encouragement in his aeronautical studies. It was at about the time Miller graduated from high school that he told his father of his intention to become an airplane designer and engineer.

The elder Miller, while sympathetic, still felt that his son should take advantage of the very rich tenor voice which he possessed and during the summer after his graduation he was required to study singing with a local teacher. The publication of the technical article in *Aero and Hydro* was impressive, however.

And the magic of seeing his name in print was enough to make Miller lose interest in music. Probably the father, too, was dazzled by this brilliance. At all events, before the end of the summer, he consented to enrollment at the Oklahoma Agricultural and Mechanical College at Stillwater as a freshman in mechanical engineering.

Miller's education was interrupted, because of financial reverses, after he had been at college for a year. During this period he taught carpentry and woodworking in an Indian School at Seminole, Okla., and also when twenty-one years old, was married to Mary Davis of St. Louis, who had been a coed at Oklahoma A. & M. Miss Davis seems to have been a thoroughly modern young woman, for she enabled her husband to save enough to enter the Missouri State University the following fall and then obtained a job for herself. Miller was made a student instructor and somehow they managed to get along. During his junior year he was requested to design an air propeller for a boat and thus became interested in the propulsion of airplanes. With the assistance of Dr. Earle Raymond Hedrick, now at the University of California, he worked out a complete mathematical and physical theory for the air propeller. This served later as the basis for a master's thesis at M.I.T. (Miller, like so many of the other directors of research, eventually attended that school) and

even to-day the material in it is useful to him in his laboratory.

The World War interrupted Miller's studies and he served in the Engineers' Reserve Corps. But in June of 1920 he was graduated from Missouri with the degree of Bachelor of Science in Engineering. After graduation Miller accepted a position with the Westinghouse Electric and Manufacturing Company and worked on the "Micarta" propeller for airplanes. This is the propeller fashioned from a laminated form of Bakelite and is the type used by Maitland and Hagenberger on their flight to Hawaii. The education of Miller was not yet completed, however. While at the Westinghouse plant at East Pittsburgh, he wrote to M.I.T. regarding post-graduate courses in aeronautical engineering and was astonished, after he had supplied data regarding his qualifications, to be offered an assistantship in the department of aeronautical engineering. The post paid $1200 for the school term. Mrs. Miller felt certain that she could find employment in Boston for herself, and Miller's father, by now entirely convinced that his son had chosen the right calling, announced that he was again financially able to be of some assistance. Consequently, overjoyed at the full measure of good fortune, the Millers set out for M.I.T. He entered as an assistant and student in the graduate department of aeronautical engineering under Professor Edward P. Warner, now Assistant

Secretary of the Navy for Aviation. He remained at the Institute for two years and while there designed and supervised the construction of the two new wind tunnels built during 1921 and 1922. The larger of these has since been moved into the recently dedicated Guggenheim School of Aeronautics at the Institute.

By 1922, the year he received a degree as Master of Science in Aeronautical Engineering, Miller was already widely known in technical aviation circles. Among those who had heard of his work were the executives of the Curtiss Aeroplane and Motor Company, of Garden City, and they asked him whether he would assume direction of the company's research laboratory. Feeling that the opportunity was exactly what he needed, Miller accepted and when he arrived at the plant was informed that the company desired to develop a racing plane which would break the world's speed record by a wide margin.

Why speed? Was the Curtiss Company merely seeking publicity, anxious only for headlines in the newspapers? Was this merely another indication that the twentieth century had gone mad in its effort to obtain speed? Not at all. The Curtiss people wanted to develop fast racing ships so that the knowledge gained in making them could be used, if war came, for fighting ships which would dive on an adversary at terrific speed and dart away. By building machines for the Pulitzer air races the com-

pany would force its research, design, and motor staffs to extend themselves to the utmost degree. Thereby the development of aviation would be more rapid. The twentieth century is, of course, the age of speed. Each night, it has been said, there are shipments aggregating tons vitally needed in distant parts of the nation. Business men stand ready to pay almost any sum to have them delivered within a few hours. In time, the airplane will do this. Each year the Curtiss Company has turned out faster ships, motors that are more powerful for their weight, planes that are stronger. In '49 it took a wagon train six months to cross the continent but on June 23, 1924, Lieutenant Russell L. Maughan flew from New York to San Francisco between dawn and dusk.

Miller, taking over the Curtiss aerodynamical laboratory, was given a part-time assistant, Michael Watter, who has since become design engineer for the Chance M. Vought Company. He found it necessary to install a large amount of new apparatus, to recalibrate and explore the airstream of the wind tunnel and to develop new methods of testing bodies with low resistance. He had, in fact, to start far down among the fundamentals, for in building racing planes the company had to know before the ship had been completed, even during the preliminary stages of design, what its performance in the air would be. What was the use of going on with some design that would not develop higher speed or be perfectly

safe? So scores of wings were built, often of "plaster of Paris" by a secret process so that they could be fashioned and refashioned quickly. Fuselages were made of wax or plasticene so that their shape could be varied in the laboratory. Scores of models were tested in the wind tunnel—for resistance and for control.

In general, the problem was divided into two parts: the development by the airplane engineer of a machine of low weight, great strength and, above all, low resistance; second, the development by the motor expert of an engine of great reliability which would exceed all others in power output per pound of weight. It was work that required vast technical knowledge. First the formulæ were developed in the laboratory under the direction of Miller. Then the engineers evolved a little model, mathematically perfect, which represented the design produced by research on the motor, wings, fuselage, landing gear, controls. This model was mounted on the top of an extremely sensitive balance in the wind tunnel where atmospheric conditions and pressures could be simulated at will. Finally, careful records were made and checked against formulæ and design specifications. The result was an astonishingly accurate forecast not only of speed, but of control and other characteristics of the finished plane to be built from the wind tunnel model.

Many important facts were developed as a result

of the numerous tests. The resistance of the under-carriage of airplanes, for instance, was found to be a large proportion of the total drag against the wind and in consequence a vastly simplified landing gear was developed. The conventional water-cooled engine radiator also offered great resistance and has finally been eliminated from racing planes by developing a covering for the wings made of two thin corrugated copper sheets between which the water could be pumped. The resistance of the landing wheels, even, had to be greatly reduced, and new streamline designs were fashioned. The wind tunnel experiments further disclosed that a powerful rudder was a most important safety factor and that designers, using the old rule-of-thumb method, had generally been making them too small. The Curtiss aerodynamic laboratory developed new data governing the size of control surfaces.

So much for technicalities. In developing the Pulitzer racers the final design of the racer was each time worked out and a one-twelfth size model tested. From this test the speed of the machine was predicted *within less than one per cent.* In 1922, Miller's first year with the Curtiss Company, Lieutenant Maughan piloted the racing plane at 222 miles per hour against a predicted speed of 221.

In 1923 the company had succeeded in stepping up this speed to 248 miles per hour. Back of each mile added to that record, the fastest speed at which

a human had ever traveled, was months of patient work of the laboratory staff, first in theoretical calculations and design layout on drawing boards, then with miniature models tested in the wind tunnel, and finally building the "mock up," or full-scale, model of the racer in skeleton form. In the design of each new racer, the laboratory staff were fighting a battle of compromise between speed and the factors of safety and control. Test pilots, practical mechanics from the factory and flying field, contributed their bit, all members of a team playing against those ancient rivals of speed—air resistance and old man Time.

The "mock up" of the 1925 model had been pruned down until air resistance was cut to a minimum—in fact, it looked more like a winged projectile than anything else. The model was then turned over to the shops, the factory rushed out the finished job, and another "mystery" ship was slipped into its hanger under the cover of darkness. There expert mechanics tuned up the engine for speed trials to be run over a measured course in the still air of a morning, just after dawn.

It is a thrilling scene, the first trials of a new racing airplane. The ship is trundled out of the hanger just as day is breaking, speed trials are made in undisturbed morning or evening air. The pilot, after inspecting struts, wires, and control, warms up

the engine at the cry of "contact" as the mechanics spin the propeller; the slim racer pulls at the blocks.

Timers, starters, and instrument men are in the pylons, standing like sentinels at the markers at each end of the course, where electrically synchronized stop watches have been set up. Miller and his staff dart about, assisting in the last minute technical preparations for the great event. The laboratory staff are kept busy to relieve the tension while waiting for the timers to turn in their figures after the run. The speed trial figures will tell the final story of the success or failure of their efforts. The fruit of months of labor stands on the runway ready to take the air. The slim graceful racer starts down the field and, like a bird poised for flight, waits for the starter's signal for the take-off. At the wave of a flag, the pilot "gives her the gun" and the plane, gathering speed, literally jumps into the air, out of a cloud of dust. The pilot circles the field a couple of times and then straightens out for the first run down the speed course with the wind. As he passes the Pylon, the pilot zooms to kill speed at the finish of the run, makes a turn and a reverse run down the course against the wind. Several runs are made in each direction with and against the wind, if there is any, and the average of the runs is taken as the mean speed. The trial runs over, Miller, his staff, the pilot, mechanics, even executives of the company, rush pellmell back to the flying office where, after

instrument calibrations and other corrections are made, the average speed is announced—264 miles an hour! The 1925 Curtiss racer has more than lived up to expectations and confirmed the predictions of her designers.

A five-year record of the Curtiss racers in the Pulitzer Trophy Race, the speed classic of aviation, has been first and second place in each successive annual event from 1921 to 1925. There's a record for you! A record which bears testimony to the achievement of aeronautical science, to the skill of pilots in the mastery of air, to the practical knowledge of the craftsmen in the shop, mechanics of the flying field, and last, but not least, of the justification of the faith in their men, which company executives like C. M. Keyes and Frank Russell demonstrated when real backing with generous appropriations for the work was required.

Miller's story is important, it seems to me, partly for the reason that he has demonstrated the value of fundamental research in an industry just beginning to grow. He feels, himself, that his best work with the company has been in connection with the improvement of design theory and methods. Soon after taking his position he consented to serve as a contributing technical editor of *Aerial Age*, one of the journals he had read so eagerly as a boy. In this capacity he has written many technical articles dealing with stability, performance, wind tunnel theory,

and propellers. These have been of great value to the entire industry. By now, of course, Miller is an experienced pilot. He knew the theory of aviation before coming to the Curtiss Company and had been up a great many times. In 1922 he was taught to fly at the Curtiss school and learned to solo in three hours, a remarkable record.

When, with the entry of the United States into the World War, the army and navy demanded huge air fleets, it was the engineers of the automobile industry who offered their services. The names of such outstanding automotive engineers as Vincent, Marmon, Ford, Hall, Kettering, and many others are linked with those days and among their contributions to wartime aviation were the Liberty and Hall Scott engines, Delco ignition, the Micarta propeller, and a number of accessories. The debt is now being repaid, for streamline models of automobiles are now being tested in the wind tunnel and research looking toward the reduction of wind resistance in motor cars is being carried on.

Miller, like some of his older brothers in the field of research, is a pioneer. The Curtiss Company was the first concern to begin a thorough research program. That it has paid, in money, is demonstrated by the present standing of the company and by the fact that Curtiss planes, all developed in their early stages in the laboratory, have captured seven world's records. In the future, it is certain, research is to

play an even greater part in the further conquest of the air. Already the Daniel Guggenheim Aeronautical Laboratory, established at M.I.T., is functioning. A vital part of the equipment of the new laboratory there is one of the world's largest wind tunnels designed by Miller in his student days at the Institute.

Who knows but the same adventurous spirit of the pioneer and the explorer in the unknown realms of aeronautic science is welded into that piece of apparatus through the painstaking labors of its youthful designer. There in that institution of learning it may serve as an inspiration to the workers of the future who, in building the foundation of a new industry in the bed rock of scientific research, will leave the record of their achievements—milestones on the road of industrial progress—as Miller has done, buried under a mass of technical data—in laboratory notebooks.

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NEW YORK

Publishers of BOOKS and of HARPER'S MAGAZINE

Established 1817

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